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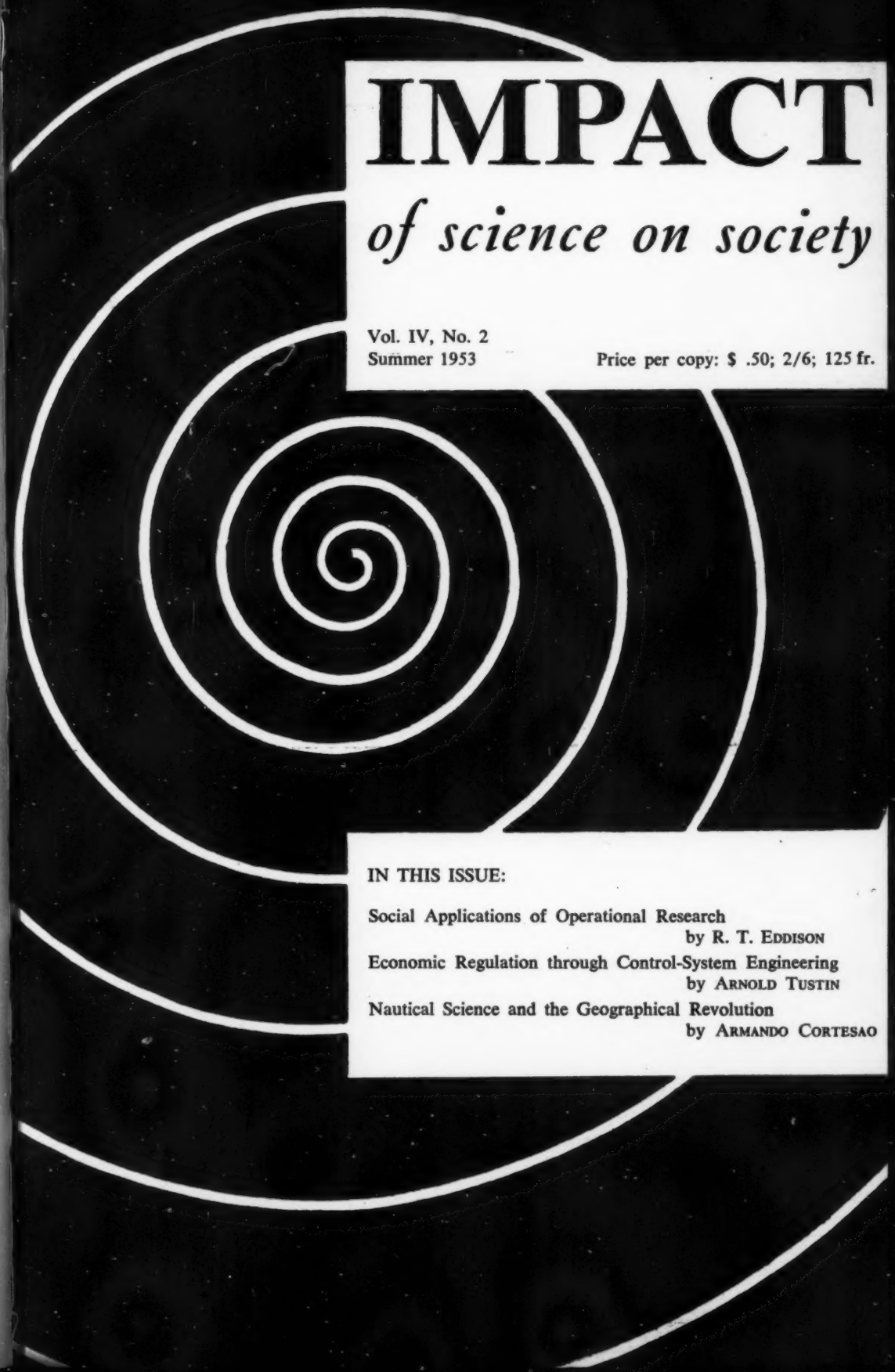

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SOCIAL APPLICATIONS OF OPERATIONAL RESEARCH

by

R. T. EDDISON

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INTRODUCTION

Social evolution has much in common with biological evolution. Both started with simple organizations and have gradually developed complex ones. In both, such development was at first a step by step process occurring by chance. If a step proved advantageous, the resulting body prospered and formed a base for further change; if the step proved to be a mistake, the resulting body faded away with no very serious consequences.

Biological evolution continues to be a step by step process, unplanned except perhaps for man's efforts to control it in domestic animals and plants. Since man first developed reasoning powers, however, social changes have not always been entirely due to chance but have often been purposeful and consciously directed towards some definite end; and to achieve this, it was necessary not merely to see the end at which to aim, but also to have a guide to show the way to that end. Such a guide is provided by experience. At first, changes, even if consciously directed, continued to take place only in relatively simple steps and the experience of the individual was sufficient. Gradually, however, the complexity of life became greater, so that the experience of the individual became less and less adequate as a guide, for it could only be a small part of the whole fund of experience. Thus it became necessary, in addition to having a consciously determined goal, to make a conscious effort to analyse the past experiences of many individuals in order to obtain the best available guide towards the future.

PRE-WAR DEVELOPMENTS

All this was a process of gradual evolution, but it is profitable to consider in a little more detail the period of 50 years or so before the outbreak of

the second world war. These 50 years were a period of intense activity, but in this connexion three lines of development are of particular interest.

Firstly, the whole social and industrial structure was rapidly becoming much more complicated. At first sight this was perhaps most obvious in industry, where changes no longer took place by small, simple steps but often by big leaps involving vast capital investments; trial and error methods were no longer applicable, for error might spell disaster not just for one man but for thousands. In other words, it became vital that decisions for action should be the right ones; once acted upon there was no going back.

This development, however, was not confined to industry; it was happening in all types of social organization and, above all, in government itself. This can perhaps be most vividly represented by a brief reference to the English scene. At the end of the eighteenth century in Britain any infringement of "the sweet majesty of private life" was still most strongly discouraged; because of this, administration was supported cheaply—much more cheaply than in any other country of equivalent size and importance in Europe. Contrast that with taxation today. Government has become big business, and it cannot afford mistakes because they will be big.

Secondly, scientific methods, albeit in a limited context, were tried and found useful in the field of industrial administration. It is convenient to trace the beginnings of that movement to the work of Taylor at Bethlehem in the U.S.A., who by meticulous observations was able to effect great savings in the amount of work done to achieve a given end; this was to develop into time and motion and similar types of studies designed to improve the efficiency of the utilization of resources. Careful observation by itself does not, of course, make science; nor is the ability to observe an attribute of scientists only; it is, however, a fundamental requirement of scientific method, and this work grew into what became known as scientific management.

The third line of advance during this period, which is of particular significance here, was in the field of science itself, namely the development of techniques for analysing complex data. These techniques were applied in the first place to the so-called "inexact" sciences—biology and agriculture. In these branches of science, experimental work had for long been bedevilled by the number of variable factors which were nearly always involved and by the complexity of the interactions between them. It was therefore difficult, and usually inappropriate, to hold other factors constant in order to study one. But the development of statistical methods of experimental design and analysis enabled giant strides to be made in these sciences.

Just as it has been possible to liken the process of development of social structure to biological evolution, so it is possible to find much in the resulting social organization which bears a close resemblance to features of biological organisms. These features include: (a) the concept of wholeness, in that

it is not possible to take one part away from the whole and consider its function entirely independently; (b) segregation, in that where a certain degree of structural complexity is reached, further progress is only possible by passing from a state of undifferentiated wholeness to some degree of differentiation of the parts; and (c) a need for progressive centralization arises as a result of progressive segregation.[1] Thus it is not surprising that the methods of scientific analysis which can resolve problems arising in the study of living biological organisms may also be useful in the study of living social organisms.

THE WAR AND THE BIRTH OF OPERATIONAL RESEARCH

By 1939, therefore, it may be said that the position was ripe for development. There was an urgent need for a new mechanism to provide executives, whether in industry, government, or anywhere else, with a better understanding of the workings of the organization they controlled, so that they could the better predict the effects of any changes they might make; there were the means of developing such a new mechanism by making use of recent advances in science; and there was a willingness to try such new means, bred from the earlier, successful results of the limited application of scientific methods. The outbreak of the second world war interrupted the natural course of this development; but it introduced at the same time a sharp stimulus towards the use of scientific methods to study the workings of the military machine.

One of the earliest uses of scientists for this purpose was apparently quite accidental. When some of the first radar equipment was being delivered to the troops engaged on British defences, scientists, who had designed the equipment, were sent to hand it over and explain how it worked to the troops who were to use it. They stayed long enough, however, to develop their own ideas on the further problem of how radar should be used to be most effective. It was soon realized that these scientists had made valuable contributions towards completing an operational plan which would make the best possible use of the limited resources available at that time for the defence of Britain against air attack. It was not long before teams of scientists were being established to advise on the best method of employing military resources in many other situations throughout the British, and later the United States, armed forces. Since these scientists were studying problems concerned with the use of resources in military operations, they came to be called operational research teams (in the United States, operations research teams).

1. "General System Theory", by L. von Bertalanffy. *Brit. J. Phil. Sci.*, Edinburgh, Aug. 1950, 1(2) 134. See also *Social Evolution*, by V. Gordon Childe. C. A. Watts, London, 1951.

The present study is concerned with the social implications of operational research and not with the history of its use during the war. The story in its early development, however, provides a clue to the proper understanding of its potentialities, and it is therefore appropriate to describe very briefly one example of its war-time application.

An operational research team was sent to study the use of depth charges in anti-submarine attacks by British naval forces. The team found that the depth charges released on a diving submarine were always set to detonate at a given depth (100 ft.). On making enquiries they found that this was a long-established rule and that the reason for it was that at that depth the pressure of the water caused the detonation to have its greatest power. The team, however, were doubtful about the correctness of this technique; for, although the detonating power might be greatest, the use of this setting involved delay in launching the attack, since it was necessary to wait long enough for the diving submarine itself to reach that depth. Furthermore, during this period of delay the diving submarine might turn in any direction, unknown to the attacker, and might thus be far from the point of detonation.

The scientists therefore employed probability theory to calculate the chances that the submarine would be within lethal range of the detonation for various settings of the depth charge, assuming in each case that, once submerged, the submarine might follow any course both vertically and horizontally. As a result of these calculations it became clear that the depth charge setting then in use was far too deep; it was shown in theory that by reducing the setting to 35 ft. the chances of a "kill" would be greatly increased. Then came perhaps the most difficult part of the operational research team's work; namely to have their theoretical solution applied. This was no easy matter since they were striking at the roots of rules of action which were hallowed by years of tradition. Thanks, however, to the very close contact between the scientists and the executive commanders, the proposed new rules were given a grudging trial. The scientists' suggestions were wonderfully vindicated; in the first week that their new rules were used the number of sinkings reported increased almost exactly in the proportion they had prophesied.

The early successes of operational research during the war can be attributed to a number of causes, of which four appear to be of outstanding importance; they require some further discussion, in order to give a clear picture of what operational research is and how it works, without becoming involved in formal definition. These four are: the medium in which operational research was being carried out (i.e. the armed forces); the appreciation of the need to improve performance; the application of scientific methods to resolve problems in the field; and the close link between the scientists and the military commanders on the spot.

The important point about the armed forces was that they were very large

organizations with rigid codes of conduct laid down from top levels of command for dealing with almost every situation which might arise. Furthermore, there were many situations which repeated themselves in all essentials over and over again. Since the time that these situations had first been recognized and rules of conduct laid down for dealing with them, many of them had changed; and, as is to be expected, the rules of conduct had not always been revised to keep pace with those changes. There was, therefore, inevitably a very fruitful field for the application of new methods to analyse these situations and to determine afresh the best responses to make to them. And when these analyses suggested new rules of conduct, the application of the new rules could often produce impressive results because of the repetitive nature of the situations being studied. It should not be supposed that vast organizations with set rules are an essential medium for successful operational research, but they create the conditions in which the most spectacular results are to be expected.

Operational research is essentially practical. It is carried out with the definite purpose of finding ways to improve the running of a particular organization; and this is where it differs from academic researches which may cover similar territory, but without the spur of practical application. This means that operational research will never be successful, indeed it will never be undertaken, unless the need for it is seen. The executive responsible for the organization must not be satisfied with what he is doing: his level of aspiration must exceed his level of achievement, so that he is constantly seeking new ways of improving performance and is receptive to new ideas and suggestions.

Operational research is a challenge to established beliefs, customs and traditions; it strikes at complacency. It received a tremendous impetus during the war when it became apparent that so many of the established methods just would not work. From sheer necessity operational research was given a chance. One of its biggest tasks now is to maintain that impetus and encourage among executives that constant ambition to improve, without which it cannot thrive.

This has an obvious bearing on the further development of operational research in peace-time, and it links up with the remarks made earlier that a new approach to executive problems was made necessary by the increasing complexity of the social and industrial structure.

SCIENTIFIC METHODS

The essential constituent of operational research which justifies the title "research" is that it develops new knowledge and new ideas, and it does so by the use of scientific methods. It is not easy to define what is meant by

"scientific methods" and it is not proposed to discuss it at length here. It may be said, however, that scientific methods involve careful observation, analysis of the data, and deduction according to a logical process to determine functional hypotheses; such hypotheses may be checked, where necessary and possible, by means of experiment. Laboratory scientists start with experiments to provide their initial data; operational research scientists start with data from the field, but their methods and objectives in dealing with the data are similar.

Every decision made by every executive in every organization is based to some extent upon past operational data. Most of such decisions are based upon the unconscious collection and analysis of the data which have been thrown up in the course of the life of the individual who makes the decision (or the group of individuals where it is a group decision). This process is called experience. But experience is commonly unreliable; not only has it too narrow a base, but it tends to be biased in its selection of the data from the past. Scientific methods can remove that bias and ensure that the data are properly treated to provide a true picture of the past; they can then determine by a rational process what rules appear to regulate the system being studied, and hence show what is the probable outcome of any particular action; furthermore they can provide a measure of the "imponderables" by calculating the probability of abnormal occurrences.

That does not imply, however, that operational research provides the answers to everything. On the contrary, it is limited in many ways. For example, it has been said that it determines functional rules by a rational process of analysis. But in any given situation the number of rules which might apply is infinite, and it is quite impossible to apply scientific methods to test them all. In practice, therefore, only one, or a few, of the most likely rules is tested against the facts; and the decision as to which rules are most likely will be made as often from experience, intuition or inspired guesswork, as from the analysis of the data. There is nothing so practical as a good theory which may originate from executive experience and judgment, and the function of the scientific analysis is then to enable one to determine which of the theories thought worth testing fits the facts best, and how well it fits them. A further important function of the scientific method is to show in what ways the data are inadequate and to suggest how any necessary further data may be obtained. But however exactly the hypotheses are tested, one cannot ever be sure that there is no other possible hypothesis which would fit the data as well, or even better.

A second limitation on operational research arises from the time factor. The executive has to make decisions on the best information available to him and he cannot postpone making them until the perfect answer is available. The data may often be insufficient or the known scientific methods inadequate for dealing with any particular problem. Operational research,

therefore, can only advise what is most likely to be the best answer in the light of all the circumstances at the time the decision is required. Later information may in fact show that the decision made was not the best one, and it is a feature of operational research that it should test decisions by further studies after the event.

CONTACT BETWEEN THE SCIENTIST AND THE EXECUTIVE

If the executive is to look to operational research as an aid to his experience, the operational research team must obviously work very closely with him to gain his confidence and to have a proper understanding of what is in his mind. This, therefore, is another ingredient of successful operational research, and it was present to an exceptional degree during war-time when the research workers could often share a common life with the military commanders in the field. This is perhaps the most important feature of operational research that entitles it to be considered as something new. The operational research team must identify itself with the objectives of the executive for whom it is working; and, indeed, its field of action is limited by the responsibilities of that executive. It may be able to point out the most suitable objectives but it cannot decide on moral issues. There may sometimes be conflicting interests within an organization, and great care may then be needed in deciding the precise executive level with which the operational research team is to work if the best advantage is to be obtained from it. Thus the objective approach of scientific analysis may be the means of bringing together separate departments whose individual interests may be opposed. Again, it is a common experience of operational research workers in industry that the workers will welcome and respect investigations and policy recommendations which arise from a scientific study whereas they will reject similar action proposed by "the management".

In this respect, therefore, operational research, if properly handled, may have an important role in bridging the traditional distrust between workers and managements, with far-reaching social consequences.

OPERATIONAL RESEARCH IN INDUSTRY

With the end of hostilities, the scientists who had done such successful work during the war were naturally anxious to apply the techniques they had developed to the arts of peace. As we have seen, there were reasons for saying that industry at the outbreak of war was ready for some such development. During the war, many industries had inevitably to mark time, and the years immediately after it were a period for decisions on the grand

scale regarding industrial expansion. There were, therefore, many suitable opportunities for the application of operational research, and in the United Kingdom much work has been and is being done, especially in the larger industrial organizations, in the nationalized industries, and in co-operative research. The types of project involved cover a wide range, from small investigations affecting decisions at the level of the shop floor to big studies of industrial strategy. It is not possible to catalogue them all, and the examples which will be discussed here have not been chosen as representative but simply as illustrations of special aspects of operational research with particular reference to its social, rather than purely industrial, possibilities.

The first example is one concerned with the provision of equipment in industry. In a heavy engineering shop a number of bays were situated on either side of a single rail track. A great deal of traffic was necessary to move heavy parts in the process of manufacture from one bay to another; with increasing production, congestion on this line had been excessive, and long delays in obtaining transport were seriously impeding production. There was no room for a second track beside the existing one and the solution to the difficulty proposed by the management was an expensive rebuilding of the shop to enable another line to be installed. Before this work was undertaken, however, an operational research team was asked to investigate the position. They made a detailed study of the movements which had to be undertaken, each movement being broken down into its component parts—waiting for transport to arrive, waiting for crane, loading, movement on rail, unloading, etc. All these parts were timed and the causes of all delays were investigated. From this study the team determined a functional rule so that they could show how the efficiency of the system would be affected by changes in its organization. They predicted that, if certain changes were made and a second locomotive supplied, the existing single line would be more than adequate for the requirements of the shop. Their proposals were adopted and it was found that the times taken for transport after the change agreed very closely with the times the team had predicted.

This is a simple but clear example which illustrates how operational research can sometimes provide a better answer than experience. The line, as it was originally operated, was inadequate and the cure suggested by experience was to build a second line. Careful collection and analysis of data, coupled with the formulation of a functional rule, led to the conclusion that a cheaper solution was possible, and thus the company was saved a heavy outlay on a new line.

BALANCING THE USE OF RESOURCES

The next example concerns a study made by the British Iron and Steel Research Association into the discharging of imported iron ore from ships at British ports. The problem as it was originally put by the executive was: how can the turn-round time of the ore-ships in port be reduced? The first task of the operational research team was to consider whether the problem had been correctly stated. It was quickly realized that it had not, and the problem was recast in the following form: what is the best type of equipment to install at ports and how should it be operated so as to minimize the total cost of discharge, including both the actual cost of unloading and the time the ships spend in port? When so formulated, the problem can be seen to be one of balancing the use of one type of resource against another, a very common type of problem in this sort of research.

In this instance, the resources to be balanced were on the one hand the port unloading equipment and on the other the ships themselves. If ships do not arrive—as they do not—on a schedule so arranged that one ship is ready to start unloading precisely at the moment that the previous ship has finished discharging, it is clear that there must be occasions when either there is no ship in the port and the unloading equipment is idle, or there are ships waiting to be unloaded until previous ships have been discharged. In practice there will certainly be a bit of both; the problem is, what is the optimum point of balance between idle equipment and idle ships? The greater the capacity of the unloading equipment provided at the port for a given tonnage throughout, the greater will be the idle time of the equipment but the less the waiting time of the ships.

The kernel of the problem lies in the application of the mathematical theory of queuing. To calculate the amount of waiting time of ships from pure theory, the following information is needed: (a) the queue discipline; (b) the distribution of the arrivals of the ships in time; (c) the service-times of the ships (i.e. the time a ship occupies a berth while being discharged); (d) the number of berths; and (e) the berth-occupancy (i.e. the proportion of total time that the berth is actually occupied by a ship being discharged). The berth-occupancy can be defined as the ratio of the actual annual tonnage to be handled to the theoretical tonnage that the equipment as operated could have handled had it been continuously supplied with ships to discharge; it is therefore a measure of the idle time of the equipment. The service-times of the ships will also depend upon the unloading installation (the number and type of machines and the way they are manned) and, in addition, on the type of ore and the size and type of ship being discharged. A number of steps in the research can therefore be broadly distinguished:

1. The determination of the effect of the unloading equipment, the type of ore and the type of ship on the rate of discharge. This could only be

obtained from a study of actual discharge rates achieved at various ports, and in order to do this the data for discharging nearly 3,000 cargoes were collected and analysed. On the face of it this would appear to require a standard statistical three-factor analysis of variance. In fact, owing to the large variety of ships and ores involved, *ad hoc* methods of analysis had to be devised to obtain a sufficiently reliable solution to the problem.

2. The arrival distribution of ships at port was determined from the same data. This was found to differ little from a statistically random distribution, which was fortunate since this is the simplest form of distribution to deal with theoretically.
3. A theoretical calculation was made to determine the length of waiting delay to ships that was to be expected for various values of the berth-occupancy and for given service-times.
4. This theoretical solution was compared with the actual delays experienced, as determined from the same operational data of the 3,000 arrival cargoes. A reasonable agreement was found.

This part of the research, therefore, provided a mechanism for predicting what the effect of varying the type of unloading equipment and the method of operating it would be on berth-occupancy and the turn-round times of the ships. This was, however, still insufficient to determine the best point of balance, since it was impossible to add together unloading equipment's time and ship's time. It was therefore necessary to convert both these times into a common measure, and this was done by an economic analysis of the costs of building and operating different types of unloading equipment, and of ships. Thus it was possible to show, for various annual tonnages, what port equipment should be provided to minimize the sum of the costs of operating that equipment and the costs of keeping ships waiting in port.

This example illustrates a number of lessons with regard to operational research. Firstly, it falls clearly within the definition of operational research. It was based on a study of actual operations, and scientific methods were used to analyse the data and to derive from them a logical rule for predicting the effects of change, and it thus provided the executives with a basis for deciding what equipment it would be best to install. Secondly, it illustrates the concept of wholeness in that it was not possible to consider the unloading installation in isolation. For example, the type of ship had to be examined at the same time; and the social consequences of varying the unloading process on the workers employed had also to be considered. Finally the problem illustrated the concept of segregation. Owing to the complexity of the whole operation, the cost of the unloading process and the cost of ships' time are borne by separate authorities; and one of the practical difficulties of the research was to reconcile their conflicting interests.

SOCIAL QUEUING PROBLEMS

Problems of queuing arise in many everyday matters and a considerable body of mathematical theory has been built up concerning them. In so far as their solution rests purely on theory, it is not operational research; but this subject provides a good illustration of the difference between theory and practice. The theory requires that certain factors should have one of a number of clearly defined values. For example, theoretical solutions are possible if the input distribution is random or constant; but theory cannot make an adjustment if the actual input distribution is only slightly different from random; so the only approach in this case is to derive a theoretical solution assuming true randomness and then compare the expected queuing so calculated with the actual queuing that can be measured. If the expected and actual values are in sufficient agreement, the theoretical model may be used as a basis for prediction. If. however, that agreement is poor, it may be necessary to employ an empirical approach instead of a theoretical one.

An example where this occurred is a study that has been made of appointment system in hospital out-patient departments in Britain.[1] Out-patients visiting hospitals may often be kept in the waiting room for more than an hour before seeing the consultant. This state of affairs has arisen because the overriding consideration has always been to ensure that the time of consultants is never wasted. The waiting represents a serious loss of patients' time, however, and also requires an excessive provision of waiting-rooms. This is therefore another balancing problem, to solve which it is necessary to know how the idle time of doctors is related to the waiting time of patients. Studies were made of punctuality and consultation time, and the distribution of these two functions was found to be of such a form that the mathematical theory of queues did not provide a general solution. Quasi-empirical, instead of theoretical, methods were therefore used to derive a curve relating doctors' idle time to patients' waiting time for varying numbers of patients present at the start of the clinic. It was found, for example, that if two patients were present at the start of the clinic and if succeeding appointments were at intervals equal to the mean consulting time (in that case five minutes), the doctor's idle time would be less than six minutes for the whole clinic, and the patients' average waiting time would be nine minutes.

While the solution arrived at shows the relationship between the doctor's idle time and the patients' waiting time, it does not put any value on either. Unless some such evaluation is made, however, it is impossible to decide on the correct point of balance between the two. It can fairly safely be said that

1. "Appointment Systems in Hospital Out-patient Departments", by J. D. Welch and Norman T. J. Bailey, *The Lancet*, London, 31 May 1952, p. 1105.

the value of doctors' time is not infinite and that of patients is not zero, the assumptions that seem to lie behind all too many hospital appointment systems; it is more difficult, however, to make any positive statement about these values. In the study here described it was quite arbitrarily suggested that a doctor's idle time of six minutes per clinic and a patient's average waiting time of nine minutes would be a fair point of balance. For this particular purpose that was probably a quite sufficiently accurate assessment.

The evaluation of people's time is an integral part of many operational research problems. It arises frequently in many types of traffic studies. Thus in problems of congestion on roads, it is possible by observational studies to determine the average time taken per vehicle to cover a particular stretch of road; it may also be possible by experimental means to determine how that time would be reduced by, say, widening the road. It is not, of course, possible to carry out an experimental widening of the road, but it may be possible to assess the effect of widening by observing the effect of narrowing it; and it is also a very easy matter to determine the cost of a widening scheme. But it is not possible to say whether the widening scheme is worth carrying out unless some value can be put on the time that will be saved by it. It is a fairly simple matter to observe the numbers and types of vehicles using the road and so to determine the reduction in vehicle operating costs that will accrue from the time saved. The numbers of passengers per vehicle can also be observed, and so the saving in gross man-hours is determinable. But it is too complicated a task to put a value on those man-hours in each particular case. In such cases, therefore, some arbitrary rule must be adopted for determining the average value of a person's time from gross national income statistics.

ROAD TRAFFIC RESEARCH

Transport in general, by road, rail, or air, provides a wide and profitable field for operational research of the greatest social importance. Much research in all sections of transport is being done in many countries and it is impossible to review it exhaustively here. Interesting examples are provided by studies made by the Road Research Laboratory in the United Kingdom on driver[1] and pedestrian behaviour.

Studies of average vehicle speeds on various stretches of road before and after the imposition of a legal speed-limit show that it has very little effect on the speed of the vehicles, although it does appear to have some effect in reducing accidents. Similarly, observations show that the degree to which

1. "Road Behaviour of Vehicle Drivers", by R. J. Smeed, *Operat. Res. Quart.*, London. Dec. 1952. 3(4), pp. 60-68.

drivers obey "Halt" signs when approaching a road junction where one of these is erected depends upon the obvious need to halt. The new black and white striped marking of pedestrian crossings in Britain was introduced as a result of experimental work in the Road Research Laboratory to determine what markings made the crossings most easily visible to the approaching driver. The introduction of the new markings was coupled with a reduction in the number of crossings and clarification of the law regarding their use. These changes together have resulted in a great improvement in the behaviour of both drivers and pedestrians in observing the rules applying to the crossings.

These studies show that the driver's behaviour is governed more by the technical requirements of good driving than by slavish obedience to the law as such. This suggests that the application of legal sanctions in these matters should not so much be restrictive as to provide guidance; and it is vital that such sanctions should be prescribed on the basis of sound research, since their capricious imposition where they are not necessary will merely lead to disrespect for the law, with far-reaching social consequences. A corollary of this is that the average citizen will often respect a decision, even if he feels it wrong, as long as he knows that it has been reached as a result of logical, scientific reasoning and not merely by the whim of authority. This is of the greatest importance in all spheres of legal and governmental action. It may also have an important bearing on industrial affairs in reconciling workers and management.

COMMUNICATIONS IN INDUSTRY

It has been remarked that, as organizations increase in size and complexity, they develop a tendency towards progressive segregation of their parts while nevertheless retaining their essential wholeness. The reconciliation of these two contrary concepts of segregation and wholeness involves the problem of communication. Communication in this sense is not limited to the purely physical aspects of moving people from one place to another or of transmitting a voice by telephone or a letter by mail; it involves the fundamental process of transmitting an idea from one person's mind to another. This requires, firstly, the expression of the idea in some form of message; secondly, a knowledge of who needs to receive it; and thirdly, the establishment of the physical means of transmission, e.g. personal meetings, telephones, etc.

The expression of meaning is itself a problem affecting the whole of society. Its importance was recognized as early as the biblical story of the tower of Babel, and semantics have been the happy hunting ground of philologists down the ages. But the satisfactory communication of meaning involves more than mere words; it involves also insight into and interpretation of

behaviour which is often in itself meaningful. It requires, above all, an understanding by the communicant of the mental processes of the recipient and vice versa. Thus a mathematician can communicate with another mathematician concisely and with precision by the use of symbols and formulae which to the non-mathematician are meaningless. Even to the non-mathematician, however, such expressions remain precise; if he finds them uninterpretable, at least they are not open to misinterpretation. Not so the ordinary language of everyday life; it is appalling to contemplate the social waste that must be involved in protracted diplomacy and in litigation where the sole cause of difficulty is misinterpretation.

This aspect of communication, therefore, has a wholeness which suggests that research into it may pay vast social dividends. As a whole, however, it is too heterogeneous, and progress may best be obtained by segregating different aspects of it and studying them separately. It is encouraging that there are developments of semantic theory based on scientific study, not only of the relation of meaning to words, but also that of language to behaviour; this development is taking place particularly in connexion with communications in industry, and is, as it must be, based upon operational research studies rather than pure theory. Since misinterpretation of meaning is a cause of mistrust (just as mistrust is a cause of misinterpretation), any research which improves the communication of meaning may have important social effects by improving human relations in industry.

The exact channels of communication needed in industry can be determined from a functional study of the various parts of the industrial structure under review, and comparison with the channels which are open in practice will then enable the efficiency of the communication system to be assessed. Operational research has a vital part to play here, since it is essential that the system be studied as it is actually working, and not merely as the management thinks it is working. Such studies must include not only vertical communications up and down between top management, departmental management, foremen and workers, but also horizontal ones between department heads, between foremen, between staff and line management at all levels, and so forth. Furthermore, the best system of communication for a particular organization can only be determined in relation to its general level of culture, and this will require operational experiment.

The study of the physical means of communication to be used also provides an open field for operational research. The provision of rapid means of communication costs money, and this should be balanced against the savings in time that will result. The dis-economy of constant interruptions, e.g. by telephone, needs to be assessed, as also does the real value of meetings, including joint consultation meetings; this can be done by operational studies while they are in progress, which can lead to a better understanding of how they work and so of what action can be taken to improve them.

PRODUCTIVITY

Improvement of productivity is an important social objective, for it is a means of raising the standard of living, especially in a country with full employment. The word "productivity" is used here to include labour productivity or productivity of equipment. The development of methods of controlling productivity is a task for operational research, although the day-to-day operation of such methods once they are developed becomes the province of routine management; in just the same way the development of a system of quality control may be classed as research, but its subsequent routine operation is not.

Much has been heard of productivity since the war, but most of the research work done on it has been in the derivation of measures of productivity, though the meaning of the measures or of the use to which they can be put has not always been clear. Useful measures have been developed in the form of gross national productivity rates for an industry, or gross rates for particular companies or for particular departments within companies. Such measures are utilized for comparison with other similar organizations; for example, the productivity of an industry in one country is compared with that of the same industry in another; the productivity of a department making a particular product in one works is compared with the department making the same product in other works.

Such comparisons may often show differences which are at first sight startling, but they are valid only on a strict basis of comparing like with like; and since it is rare for even two shops in one industry to be exactly alike, let alone industries in two separate countries, it is usually only too easy to find excuses for relatively bad productivity on the grounds that the comparison is invalid. Examples of this may be seen in some of the reports issued by productivity teams which have been visiting the United States in recent years. Thus, the team sent by the British iron and steel industry gave figures in their report which showed that, in certain processes at least, the productivity in the British industry was substantially less than in their United States counterparts. It was, however, at once apparent that a large part of these differences could be accounted for by inherent local circumstances; in the iron-making process, for example, the United States industry has much richer and more consistent raw materials available. While, therefore, such productivity figures stimulate the industrialists of one country to learn from the other, part of their value in disturbing complacency on both sides may be lost if they can be too easily excused away.

Similarly in comparing different companies within a country, the crude productivity figures may often lead to false decisions as to the best action to be taken. For example, in the British farming industry, which has outstanding achievements to its credit, some farmers are inevitably worse than

others; if a distribution of productivity could be determined, it would almost certainly be not very dissimilar from a normal curve, showing up a few very good farms, a few very bad ones, and the vast majority in an intermediate position. Much energy and money is spent in harassing the few very bad farmers to improve their output; how much better it might be in terms of gross national agricultural product if this energy were spent instead on improving the productivity of the multitude of farmers in the middle range.

The point is that the objective of productivity studies should not be to compare one production unit with another that is similar but not the same, but to compare the actual productivity which the unit attains with the best productivity that it could obtain within the inherent restrictions under which it operates. For this purpose the great value of gross productivity measures is to show how the productivity varies from year to year. What is required is detailed operational research into the functioning of the works as a whole, including consideration of its social and cultural conditions. Such research could determine not only what the productivity was, but also why it was what it was. Only then could rational decisions be taken which would improve it.

INDUSTRIAL ACCIDENTS

The injury, maiming and death of workers involved in industrial accidents are a grave social and economic problem, and from a cold economic standpoint, it does not yield to a ready assessment of cost. Firstly, there is the same difficulty as with road traffic delays in the evaluation of a man's lost time; in this case, that is perhaps not quite so difficult since the earning power of each employee injured will of course be known with fair precision, but the assessment of cost of disability is more arbitrary. Secondly, there is the fact that only a part of the cost of accidents is felt by the management of the works in which they occur; the remainder, often the greater part, is a social cost. For these reasons accident prevention is often promoted by managements more for humanitarian motives than out of cold economic calculation. This is perhaps not altogether fortunate, for it might be found that the cost of accidents would well justify the expenditure of far more energy on preventive measures, especially if full account were taken of the attendant advantages, such as increased productivity, in which these measures might result.

The approach to the problem of reducing accidents has, indeed, much in common with the approach to the problem of increasing productivity. Crude accident statistics are usually kept on a factory basis; in many countries the keeping of such figures is a legal requirement. Two measures

are in common use: the frequency rate (the number of accidents per hundred thousand or per million man-hours worked in the factory) and the severity rate (the number of days lost per hundred thousand or million man-hours worked). The main use of these figures is for making comparisons, in which, as before, the difficulty arises of deciding the validity of comparison, and for watching the movement of accidents within a factory from one year to another. These figures give little clue to the underlying reasons for accidents, however, and the steps taken to avoid accidents have for the most part consisted of mechanical protection of the machine or the worker, or of somewhat haphazard attempts at propaganda, exhortation and the like according to the intuition of the responsible executive.

For some years now considerable attention has been focused on the importance of the form of organization for accident prevention within the factory, and more recently there has been a tendency towards regarding accidents as a symptom of the social climate of the factory as a whole. In one case at least a thorough-going study of accidents is being made as an operational research project. The study is not restricted only to the accidents themselves but also considers the workers—those who do not have accidents as well as those who do. A sample of entrants into the factory has been taken and their subsequent accident records analysed in relation to such factors as their entry characteristics (e.g. age at entry and previous employment), their lengths of service at times of accidents, types of accidents, causes of accidents, changes of occupation or department within the factory, sickness and absenteeism. This study has already yielded valuable information reg ding the processes of selection, induction, training and other matters which have not previously been brought together in association with the study of accidents. It indicates the type of approach that may develop when the ideas of operational research are applied to this and similar problems.

In the preceeding sections, communication, productivity and accidents have been treated separately for ease of exposition. It should not, however, be supposed that they are in practice entirely separate and dissociated. Rather must they be regarded, along with other functions, as different facets which go to make up the whole social unity of the industrial organization. Their study cannot be complete if a mechanistic approach is adopted and each function is treated separately; on the contrary, it is necessary to treat each organization as a unit and study the interaction of all functions within it. Recently published studies of three different organizations, a colliery and two companies in the hosiery trade, show how this approach can successfully be made.[1]

1. *3 Studies in Management*, by Jerome F. Scott and R. P. Lynton, Routledge and Kegan Paul, London, 1952.

THE FUNCTION OF GOVERNMENT

Just as the functioning of industry was becoming rapidly more complex in the years before the war, so also was that of government. In many branches of industry the war was a period of marking-time or of recession, but the function of government became transformed in order to meet the emergency. The war was total war in that the entire resources of the countries concerned had to be mobilized with that single end in view. Government could no longer afford a more or less benign but rather negative approach to its duties; the allocation of resources between various conflicting interests could no longer be left to the push and pull of the market; the government had to be up and doing, taking positive action to direct the national life. This growth of direct government action took place in some measure in all the warring countries; it may be illustrated by more detailed reference to some of the developments in the United Kingdom, particularly the activities of the Board of Trade—the British Government Department that was primarily concerned with the civilian aspects of industry and trade rather than with direct production of warlike materials.

The history of this department during the war has recently been published.[1] It shows what little information was available at the beginning of the war about any of the facts which were obviously an essential basis for determining the policy of the department, if it were to take executive control of the distribution of industrial resources. For example, even when output statistics were available for products, little was known about the materials essential for their manufacture, and even less was known about the buying habits and requirements of consumers. This did not perhaps matter a great deal as long as materials and labour were still moderately plentiful. When the supplies of cotton began to fall a little short of total demand, it was at first sufficient to establish a system of priorities to ensure that the war industries and exports had all they needed, and the civilian consumer took the balance; he might not get all he wanted, but the matter was not critical. Gradually, however, the war industries became more rapacious, and supplies of resources, labour as well as materials, dwindled. It became necessary to give more positive thought to the position of the civilian consumer, for it was essential to keep up morale.

One form of action adopted to deal with this situation was rationing. The primary purpose of this was to ensure fair distribution. In itself, however, it led to further difficulties, for rations had to be honoured. Furthermore, some rationing schemes—e.g. clothing rationing—covered a wide variety of articles and the public expected to get not merely something, but the particular articles they wanted. This meant that the Board of Trade had to

1. *Civil Industry and Trade*, by E. L. Hargreaves and M. M. Gowing, H.M. Stationery Office, London, 1952.

assume responsibility for seeing that the right goods were produced in something like the right quantities. It is impossible here to pursue the ramifications of these intricate and fascinating problems. They may, however, be summed up by saying that the Board of Trade was faced with a problem of striking the best balance between consumption of scarce resources on the one side and the deterioration of morale on the other. This is just the type of problem where operational research could have been invaluable if the need had been clearly seen in time. In the event, progress was made mainly by trial and error.

On the production side, it is particularly surprising that little attempt seems to have been made to estimate the effects of the policies adopted on productivity, especially on the productivity of labour which was latterly one of the scarcest resources. On the consumption side, estimation of real demand was of the greatest importance, for once there was an apparent slight shortage of some particular article, panic buying set up a self-aggravating process. For a long time, however, such estimating was mainly by guesswork for most goods, although in some cases techniques were developed for calculating demand, e.g. for coupon expenditure. Later a special consumer needs department was set up with distribution officers to watch the flow of retail trade. These achieved some success in smoothing the flow, particularly between one area or town and another, but this work was essentially a patching process, not a scientific study.

SURVEYS

An estimate of the needs and wishes of the population is a fundamental requirement for almost any government policy decision. In former times, one of the reasons for lack of data on which to base the decisions was the lack of the techniques to make the collection of such data practical. The two alternatives were either to conduct a complete census, which is a big undertaking when whole countries are concerned, or to rely on a small sample. The latter was relatively inexpensive, but until a proper theory of sampling had been worked out it was liable to be hopelessly inaccurate. One of the most important advances of statistics in recent years has been the development of a precise theory for planning sample surveys in such a way that, not only can the sample be relied upon to be representative of the whole population, but the probable limits of error, i.e. of the difference of the sample from the population, can be determined. Sample surveys are now widely and confidently used in many types of factual investigation, and are far from being regarded as cheaper but less accurate substitutes for complete censuses. The reason is that censuses involve a vast amount of work, much of which must be carried out by untrained, or at best ill-trained, workers;

there is therefore great scope for inaccuracy in the data recorded. Sample surveys, on the other hand, require relatively little of such routine work and can be carried out by trained workers with great care, so that the accuracy of the data recorded may be far greater than in a complete census. Provided the survey is properly planned, it may be not only cheaper but more accurate than a complete census.

This is now so well recognized among survey workers that when the last population census was carried out in the United States, the Bureau of the Census organized a simultaneous sample survey to check the accuracy of the complete census. Similar action was taken by the Social Survey in the United Kingdom, in preparation for a recent census of distribution. At the request of the government, it made experiments with sample interviewing techniques for collecting census data in one area of the country while at the same time trial studies, using conventional methods of postal distribution and collection, were carried out by the Board of Trade in other areas. It was possible to measure the comparative costs and relative efficiency of the two methods, and so to decide in which manner the census of distribution should be organized. The days of the full census may well be numbered.

Studies made in this way to determine the most efficient type of survey or census for any particular purpose can be classed as operational research in their own right. The routine use of well-tried survey techniques is not itself operational research, but provides an invaluable tool for it. Surveys have provided a basis for operational research to guide many governmental policy decisions during and since the war. One of the major surveys, started during the war and continued after it, was carried out for the Ministry of Food to ascertain the food consumption habits of the population. The aim was to check the effectiveness of the food rationing scheme and to ensure the adequacy of nutritional standards in all sections of the community; many adjustments of the rationing scheme were made as a result of it. Another example of similar research was a survey carried out since the war to determine what would be the demand for coal if it were taken off the ration; it resulted in the rationing scheme being retained. On the other hand, in 1949 sweet rationing was abandoned under political pressure without proper research into the position, and it soon had to be re-introduced.

There are innumerable openings for operational research in connexion with other government activities not related to shortages and rationing. The operation of health services is a good example since it probably provides as many opportunities for research as does any other social activity. Such research might be undertaken on a comparatively narrow front. For example, the work on appointment systems, to which reference has already been made, falls within the jurisdiction of each individual hospital management, although it would gain generality from being based upon observations at a number of hospitals. On the other hand, much broader issues are also

amenable to operational research; thus the question of the adequacy of general practitioner services is at present under review in the United Kingdom, and the siting and provision of hospitals could be studied in the same way. These broader issues may not fall within the competence of any one executive in countries where the health service is not organized on national lines; even so, they are problems which no government can afford to ignore. When the national health service was started in Britain in 1948 it obviously constituted a major social change. It provided an ideal situation for the application of operational research in order to determine the probable effect of the change that was to be made on the demand for the health service. Apparently, however, little was done along these lines and this failure can be regarded as one cause of the difficulties in which the new service found itself in its early days.

AN ANTHROPOLOGICAL EXAMPLE

Operational research is essentially interested in its own results and, as has already been remarked, one of its most important functions is, in addition to predicting effects of organizational changes, to make follow-up studies after the changes have been made to see if they have had the expected effect. An interesting example of the need and value of such follow-up studies in connexion with governmental activity is seen in a study of medicine murders in Basutoland.[1]

The government had begun reforms in 1937 in an attempt to tackle this problem, but they had not studied the actual situation. After the war an anthropologist was commissioned to do so, and he showed how some of the changes introduced had in fact aggravated the situation by increasing the number of lesser chiefs, instead of reducing them as was required. Moreover, it was shown that arrangements intended to reduce the imposition of unpaid tasks on the average African appeared as a higher tax demand. The fact that the reforms had been introduced from the top down caused them to be misunderstood. Thus they had in fact contributed to the feeling of insecurity which found its expression in the desire for medicines made with human flesh to give the possessor a better chance to improve or maintain his position in the hierarchy. The results of the government actions had, therefore, been the opposite of what had been intended.

1. *Basutoland Medicine Murders*, by G. I. Jones, H.M. Stationery Office, Cmd. 8209, London, 1951.

IN CONCLUSION

We have seen that the need for operational research arises from the complexity of organizations. The experience of any one man, or small group of men, is narrowly based and therefore increasingly inadequate for elucidating the underlying rules which govern the functioning of the organization; furthermore, judgments based upon such experience tend to be biased in the selection of the data which arise within it. Operational research is designed to aid the experience of a single person by adding to it the wider experience of all our yesterdays. The data obtained from every source are treated objectively to ensure that due account is taken of all; it is not, on the other hand, always possible to ensure that all relevant data have been obtained, although obvious deficiencies may be made good by experiment. Scientific methods, drawn from a wide variety of disciplines, are used to analyse the data and determine what rule or code appears most satisfactorily to explain the past behaviour of the system. Finally, that rule, derived from the past, is used to predict the probable outcome of future actions.

This study has been concerned to show the uses of operational research in social, rather than purely technical or industrial affairs, although the two are in many ways inseparable. It has not been concerned with rigid definitions of operational research, and it may be objected that some of the examples given fall within the ambit of other social sciences—such as sociology, anthropology, or economics. It is contended that, just as in the so-called exact sciences a piecemeal approach is becoming less and less sufficient, so society has an essential oneness. Operational research is not sociology; it is not anthropology; it is not economics; but it does provide methods of research in problems which are common to them all, and it brings them and other sciences together, not in academic detachment, but with the practical objective of helping the executive, at grips with live operational problems, to make the best possible decisions.

For further reading:

Methods of Operations Research, by Philip M. Morse and George E. Kimball, John Wiley and Sons, New York, 1951.

Operational Research—Its Application to Peacetime Industry, The Manchester Joint Research Council, Manchester, 1950.

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ECONOMIC REGULATION THROUGH CONTROL-SYSTEM ENGINEERING

by

ARNOLD TUSTIN

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INTRODUCTION

This article is about the causes of fluctuation of economic quantities such as employment, prices, incomes, rates of investment and so on. Any one such quantity depends on or is influenced by other quantities, and economists have to consider the complete "system" of all such interacting quantities. Such systems representing more or less closely an actual economic system are often referred to as "economic models". The chief purpose of the article is to call attention to the close analogy that exists between such economic systems or "models" and certain physical systems, such as automatic regulators and controllers, that depend on the principle of feed-back. Progress in the development of engineering control systems and in the analysis of their behaviour has recently been rapid. It is therefore worth while to enquire whether any of this progress in analysis can be transferred to economics, and so contribute to the solution of the problems of economic stabilization and prediction. Apart from methods of theoretical analysis, the analogy between economic models and physical control systems suggests a further possibility. Where, as is found in practice, the complexity of the economic system is such that its behaviour cannot be calculated, this behaviour might nevertheless be determined by constructing an analogous physical system, the behaviour of which could be observed and recorded.

To combine ideas drawn from engineering, economics and mathematics, each of which has its specialized terminology, and to present the results so that they may be understood by readers who are not specialists in any of these fields, is not an easy task. But it is useful, or at least stimulating, if specialists sometimes look over the fences that separate their gardens. It is of some importance that developments in economics should be understood

by those whose chief interest is politics or administration. To this end the precision of specialized nomenclature may sometimes have to be sacrificed in the interests of ease of assimilation, and the writer must ask specialists who may read this article to be tolerant on that account. The mathematical and technical aspects of the topics discussed have been more fully and precisely developed in a book that is to be published at an early date.[1]

THE IMPORTANCE OF ECONOMIC STABILITY

Many unpleasant features of our times, from industrial mistrust to the conflict that now divides East and West, had historic roots in the past bad behaviour of our economic mechanisms. There is a long record of under-employment of labour and capital, periodic slumps, windfall fortunes and undeserved bankruptcies. It is now generally recognized that such a state of affairs must not be allowed to continue. Governments have assumed responsibility for the maintenance of a high and stable level of employment.

If permanent full employment can be ensured independently of the special conditions of war, post-war reconstruction and armament programmes, then the social outlook will be radically different in future. In particular a principal thesis of communism will become inapplicable, namely the thesis that any economic system in which investment is determined by expectation of profit must on that account be unstable, unjust and ultimately unworkable; and that the heralding of a régime of permanent full employment is just noise without meaning. It must indeed be admitted that neither business men, trade unions, nor government departments yet take the maintenance of a permanently high and stable level of employment as axiomatic. Nor indeed do the graphs of post-war fluctuations give much objective evidence that it has yet been securely attained.

The promise of a better future which economic stability holds out, as well as the possibility of bringing to a peaceful natural end one basic assumption underlying the cold war, seem to be vast issues to hang on the solution of a technical problem. A final and unambiguous clarification of the precise mechanisms of economic fluctuations, which would carry with it the ability to predict and to regulate, does seem to have this significance. What, however, is the present state of progress towards such clarification?

It was primarily the labours of J. M. Keynes, and the publication in 1936 of his *General Theory of Employment, Interest and Money*[2] that set alight the controversy from which a new economic outlook has emerged. Whether

1. A. Tustin, *The Mechanism of Economic Systems*, to be published by Heinemann, London, in July 1953.
2. J. M. Keynes, *The General Theory of Employment, Interest and Money*, MacMillan, London, 1936.

or not what Keynes wrote was wholly true or adequate is not now so significant. He jerked economics out of a rut and set it moving again towards understanding economic mechanisms and the possibility of adequately regulating our economic life. Since the publication of the *General Theory* much further progress has been made towards the elucidation of the mechanisms of fluctuation, of the measures necessary for "nipping oscillations in the bud", and of the new problems of prices and wages that become acute in a full employment régime.

It is not apparent however that the progress made quite justifies the claim that stability is now permanently ensured, even in the sense of technical "know-how", quite apart from political "want-to". It is of immense importance that the full technical possibility should be achieved without delay, and that this possibility, together with the political determination to use it, should become accepted as no longer open to question.

This can come about only by a rapid reinforcement of economic research and statistical investigation of the right kind. There must also be some effort to make intelligible, to wider circles than professional economists, the true causes of the defective working of the economic systems of the past, and to make clear the nature, necessity and extent of the measures necessary to ensure stability and the full use of resources in future.

PARALLELS IN THE ANALYSIS OF PHYSICAL SYSTEMS

It is a curious coincidence that over the same period (since about 1935) during which economists have been giving intensive study to the "dynamics" of systems of *economic* quantities, engineers have had to tackle afresh the analogous problem of the dynamic behaviour and stability of systems of *physical* quantities. Such problems arise in particular in the design of automatic control and regulating apparatus of all kinds. It is common knowledge that during the last decade or so the technical achievements in this field have been spectacular, including such developments as the feed-back amplifier (on which radio and television depend), the automatic pilot, the guided missile, the radar tracker, the automatic gun layer, the automatic control of complete chemical plants, and scores of other developments. The basis of this technical achievement has been progress in the theory of the stability of systems. The analysis was first adequately developed in connexion with the use of feed-back in radio amplifiers, and was soon applied generally to a wide class of regulating devices.

The central theme with which the analysis of physical control systems is concerned is the avoidance of self-generated oscillations in systems of interdependent quantities. It might therefore be expected that this analysis would have some relevance or applicability to the economist's problem of specifying

the best means of avoiding economic fluctuations. In this article consideration will first be given to some of the simple concepts and conclusions of "system theory" in general, and this will be followed by a more specific discussion of economic problems and their physical analogues.

SYSTEMS OF INTERDEPENDENT QUANTITIES, AND THE CONCEPT OF FEED-BACK

To emphasize the fact that the principles in question are general principles, consider a scientific problem that is neither one of economics nor of physics, namely the biologist's problem of the variations of animal populations. Characteristic types of variation of populations are observed and it may be desired to control them. Such patterns of variation occur as the recurrent "explosive" growth of swarms of locusts, or the well-known fur cycle of Canada, where the supply of fur-bearing animals regularly oscillates between abundance and scarcity every nine years. Even the fluctuations of temperature of a fever-patient, which used to be regarded as the classical case of irregularity, are not random. They reflect the complicated interdependence of populations of bacteria with the body's defensive reactions. This problem

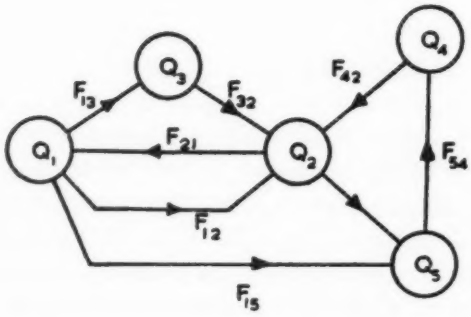

Fig. 1. Method of representing a scheme of interdependence by a diagram.

of interacting populations and environmental quantities is a useful one to consider because it puts into relief the question of what we mean by "explanation". In one sense the explanation of, say, a decline in the population of lynx in Labrador, is almost infinitely complex, and involves the accidents of each animal's life. However, the biologist is quite happy to make use of an "apparatus of description" of a much simpler kind; he takes the numbers of each species of animal, together with a few other quantities, such as some measures of the abundance of various vegetable food sources. Then, from his detailed biological observations and studies, he considers how the population of one species affects that of another, and how the quantities of foods affect and are affected by the numbers of the animal populations.

It is convenient to represent any such situation by a scheme of dependence.

For example, the biologist's problem might be pictured as in Fig. 1. The circles marked Q_1, Q_2, etc. represent the numbers of various species, quantities of food supplies, and so on, i.e. the *relevant* variables. An arrow is drawn to show where any one quantity affects any other quantity. Alongside the arrow may be written some symbol or formula expressing the effect of one quantity on another, according to the scheme of analysis to be adopted. Some of these dependences may be known, some are obvious in nature but uncertain in amount or intensity, some may be unknown, except to the extent of justifying an arrow to assert "there probably is some effect".

What could the biologist infer from such a scheme of interdependence? Of course, if the causal relations were known, exact and complete, he could hope to calculate, from a given initial state or from the recent past, the whole future development of the system. But such biological systems are very complicated and the implications of the scheme of dependence may be difficult to determine. Also, as already noted, the relations or dependences between aggregates are never exact and completely determinate. They can only represent effects as they hold in general and on the average. There are extra variations due to hazards, which, because they are omitted from the causal scheme under consideration, and because their actual origins are multiple, may be regarded as random disturbances of an *otherwise* determinate system. But if the assumed dependences of one quantity on another correctly represent the average effect, then the characteristic behaviour of the system deduced from these relationships will be meaningful, and will

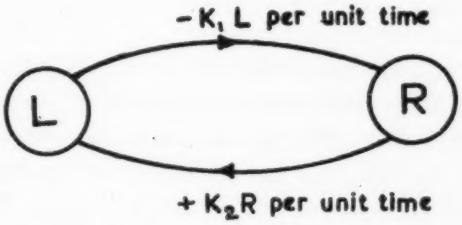

Fig. 2. A very simple scheme of interdependence, such as might exist between the populations of a predator species and its prey, supposed isolated.

make possible prediction and eventually regulation, with a margin of error that depends on the relative importance of the "law-abiding" and the "random" contributions to the variation. If the numbers of individuals in the aggregates are large, the results of the random causes of variation may be relatively quite small. Further, it is possible, given the "law-abiding" dependences, to analyse with precision the general nature of the effects of the remaining randomness.

The examination of a scheme of dependence such as Fig. 1 may enable some significant conclusions to be drawn immediately.

To illustrate one important distinction in such systems, consider the very simple partial scheme of dependence isolated in Fig. 2, assumed to represent

some of the principal factors at work in a complete scheme like Fig. 1. This shows only two animal populations, or groups of aggregates of populations, namely certain predators and their prey, or say, for illustration, lynx and rabbits, where the rabbits are the main food of the lynx, as is the case in some areas in Canada. What is to be considered is the variations of these populations, measured as departures from balance or equilibrium.

The facts to be represented are that, when there are more lynx, there will be a correspondingly greater rate of *decrease* of the rabbits, but when there are fewer rabbits there will be a tendency for the number of lynx to decrease, because of the shortage of food. Fig. 2 shows how this situation can result in a continued oscillation of both populations. The mathematically-minded will note that, for the case in which simple proportionalities are assumed for the effects mentioned, and writing L and R for the number of lynx and rabbits respectively, the assumptions are expressed by:

$$\frac{dR}{dt} = -K_1L, \quad \frac{dL}{dt} = K_2R$$

so that

$$\frac{d^2L}{dt^2} = -K_1K_2L$$

which is the same as the equation for a simple pendulum, and expresses the fact that such a system would, if initially disturbed, continue in perpetual oscillation about a state of equilibrium.

If the dependences were slightly different from these ideally simple ones, the oscillation, instead of being precisely self-maintaining, would be such that each of the successive swings was either somewhat smaller or somewhat larger than the preceding swing. In the first case the oscillation would die away and ultimately disappear. Such systems are referred to as "damped" or "stable". The second case, that of continuously increasing oscillation, is referred to as cumulative oscillation, and such systems are said to be *unstable*. Any initial oscillation of an unstable system, however small, must increase to a large oscillation, so large in fact that ultimately some additional checks are brought into play that limit its amplitude.

This elementary hypothetical system of two populations calls attention to an important concept in the understanding of the behaviour of systems. In the scheme of dependence, Fig. 2, the arrows representing the sequence of cause and effect form a *closed* sequence. In other words, the relationship between the quantities is not a simple dependence of one on the other, but of interdependence between the two; each is both cause and effect. It is evident that it is the *interdependence,* the existence of a closed sequence of

dependences, that is the necessary condition for the possibility of a continuous self-exciting or self-maintaining oscillation of the quantities, and such a closed sequence will result in oscillation if the time-relationships are of certain kinds.

Another term that has come into use during recent years to express the same notion is "feed-back". The closed sequence or loop is referred to as a "feed-back loop"; in it the quantities are interdependent. A much older term, due to the philosopher Hegel, which meant much the same, was that the relationship between the quantities was "dialectical", i.e. of the nature of a "conversation".

If, in a realistic representation of a situation, such as the problem of animal populations, a principal part is played by a closed loop such as that of Fig. 2, which however is only a part of a more elaborate system, the behaviour may show approximately the oscillatory behaviour characteristic of this principal sequence. In other cases this behaviour may be considerably modified. It is *not* generally permissible, in analyzing the behaviour, to consider each loop separately. The system must be considered as a whole.

Whether or not such phenomena as the Canadian "fur cycle" are adequately to be accounted for by the presence of feed-back loops of the oscillatory type shown in Fig. 2 is a question for biologists, on which the writer is expressing no opinion, but such interdependences must play a part in the mechanism. The surprising regularity of such cycles may well be due to the precise "turning points" being determined by the seasons, i.e. the regular annual incidence of periods of scarcity of food, breeding seasons, and so on.

A feed-back may be negative or positive, that is, the effect fed back may be such that it tends respectively either to decrease or to increase the variation of the quantity on which it operates. A negative or a positive feed-back can produce cumulative oscillation if associated with particular kinds of time-relationship.

LINEAR AND NON-LINEAR SYSTEMS

Even if a system of quantities is very complicated it is easy to deduce how it will behave in all those cases in which the dependences of one quantity on another are of the kind that mathematicians call "linear", i.e. of the kind that can be expressed by linear differential equations. In non-technical language this simply means that analysis is easy when effects are all *proportional* to their causes. In such systems only two kinds of self-exciting change are possible. The quantities may either oscillate with a constantly increasing range of oscillation, or they may change unidirectionally at a constantly increasing rate. If the time-relationships are such that neither of these types of behaviour occur, one may take it that the system is

"stable", i.e. any temporary variation dies away and disappears. The marginal case is theoretically possible but is almost infinitely unlikely.

In the case of linear systems it is also easy to calculate the effect of specified disturbances affecting the system. Even if such disturbances are random a good deal can be said about their effects.

But effects are not always proportional to causes. In fact almost no effects are proportional to their causes for all magnitudes of change. A population of rabbits might be reduced at a rate proportional to the number of lynx feeding on it, but only up to a point; for ultimately the population could not be reduced below zero, and the rate of decline could not follow the same relationship or law of proportionality as the population became very sparse. Similarly, in the direction of increase, an upper limit would come into play on account of limitations of food supply or epidemic diseases.

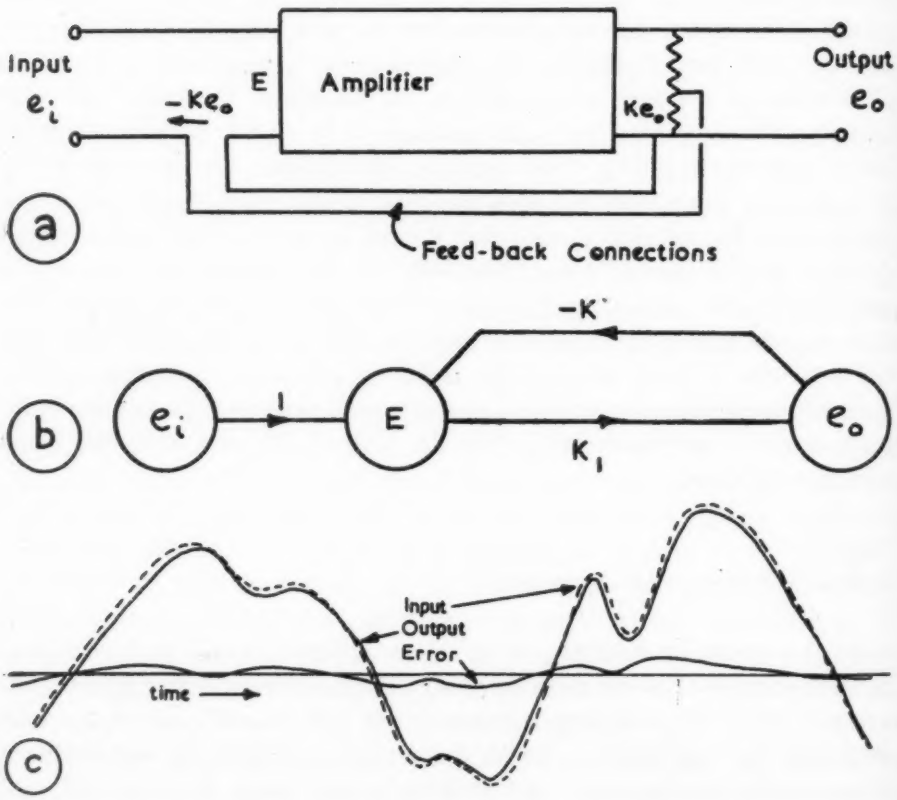

Fig. 3. Feed-back used to control the output from a valve amplifier so that it must vary in the same way as the input varies.
(a) Connexions.
(b) Scheme of interdependence.
(c) The relationship between input, output and error, during a typical variation of input.

Thus one often finds conditions in which an oscillatory system is linear or proportional for small oscillations, but as the range of oscillation increases the proportionality is lost and additional non-proportional checks come into play. These limit the range of oscillation. The behaviour may then settle down to an oscillation of constant range or amplitude, quite regular in period, but no longer quite smooth or "sinusoidal". Such limited oscillation is referred to as a "limit cycle".

In distinction from the behaviour of linear systems, behaviour that involves non-linearities is extremely difficult and at present often impossible to calculate. Its general nature can sometimes be inferred from consideration of approximately equivalent linear systems. but this is liable to result in error.

FEED-BACK IN RADIO AMPLIFIERS AND IN AUTOMATIC CONTROL SYSTEMS

The study of closed sequences in physical systems was first seriously undertaken in connexion with the use of feed-back in valve amplifiers, and this will provide a convenient further example.

Consider the audio stages of a radio receiver. The electrical signal representing the sound-waves, which is the "input" to this stage, is a small rapidly varying voltage, shown as e_i in Fig. 3. The requirement is to amplify this voltage to a proportional output voltage e_0, many times greater but varying in the same way, to operate the loud-speaker. Now amplifiers may be built that give enormous amplifications, but not with strict proportionality. It is required to control or regulate the output voltage e_0 so that in spite of this lack of proportionality it is always Ke_i where K is the required amplification and is constant. The device by which this is done is to make the amplification of the amplifier very much greater than the value K finally required, and then, as shown in Fig. 3, to *feed back* a voltage

$$\frac{1}{K} e_0$$

and balance it against the input e_i. The object, as already stated, is to make $e_0 = Ke_i$, or

$$\frac{1}{K} e_0 = e_i.$$

If this were exactly achieved the feed-back would be just equal and opposite to the input. If the requirement is not achieved, i.e. if there is an *error,* this is measured by the difference

$$e_i - \frac{1}{K} e_0.$$

It is this quantity, the *error,* that is made the actual input to the amplifier.

This makes the system, so to speak, self-correcting. Any variation of e_0, other than the variation Ke_i required, appears immediately as extra "error" and therefore as a variation of input to the amplifier in the direction to correct the error. If e_0 is, for example, a little too large, the input to the amplifier is correspondingly reduced, and thus e_0 is reduced until balance is obtained. All that is required is to make the amplification of the amplifier very great indeed. Mathematically the voltage-balance requires, if K_1 is the amplification of the amplifier itself,

$$K_1 \left(e_i - \frac{1}{K} e_0\right) = e_0$$

or

$$e_0 = \frac{K_1 e_i}{1 + \frac{K_1}{K}} = \frac{Ke_i}{\frac{K}{K_1} + 1}.$$

which becomes as close as desired to Ke_i if K_1 is made large enough.

The output voltage is caused to follow or repeat the variations of the input voltage, because the two are continually being compared, via the feed-back. The amplifier is actuated by the *error* of the output from the required value. It is only necessary to ensure that a *small* error suffices to excite any *required* output voltage to ensure that the error will in fact be small. This is a classical case of "error-actuated control". All the modern accurate, continuous, automatic controls, some of which have already been mentioned, work on this principle and have a similar basic "scheme of dependence" to that shown in Fig. 3.

The scheme of dependence is necessarily a closed sequence. All such systems are potentially capable, if precautions are not taken, of sustaining themselves in self-excited oscillation. In a radio set such oscillations are observed as the "howling" that used to be so troublesome in the earlier types.

To see that the same situation is characteristic of all automatic regulators, consider the control of the temperature of a room by a thermostat. The scheme of dependence is shown in Fig. 4. The temperature of the room is T. This is caused to determine the fuel supply F to the heater, which determines H, the heat supplied, which varies T, closing the loop or sequence of dependence. This example is informative because, in a common arrangement, the closed sequence is non-linear. When the temperature T reaches an upper limit, and electrical contact is made, that causes a substantial reduction of the fuel supply F. When it reaches a lower limit the fuel supply is increased again to the original value. This results in a perpetual oscillation of the temperature between these limits as shown in Fig. 4. The system always operates on a "limit cycle". The fact that this regulator is unstable and the values perpetually oscillate is no impairment of function for domestic use as a regulator, because the limits between which T oscillates may be set as close together as required.

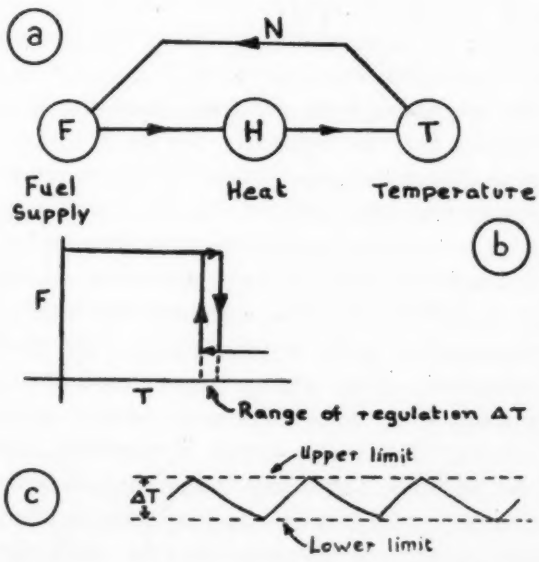

Fig. 4. The simplest form of thermostat is an oscillatory system. The scheme of dependence is shown at (a), the non-linear dependence of the fuel supply on the temperature at (b) and the resulting constant oscillation of the temperature between upper and lower limits at (c).

Many automatic controls, although they necessarily involve a closed sequence, must not involve such oscillation. The automatic pilot of an aircraft, for example, works by measuring the *error* of the direction of flight (by means of the gyro-compass) from the required direction as set on a dial. The rudder is caused to move according to the error in the direction required to correct the error. It would not do for the correction to overshoot, and then be recorrected and overshoot in the opposite direction and so on, which would result in a zig-zag path of flight. The control must be stable and "dead-beat". Means have had to be found to return the rudder to neutral

earlier, or, better still, to give it just the right sequence of properly timed movements to bring the aircraft back on to course, smoothly and without overshoot. This control of the "feed-back" as a function of time as well as of output is the essence of the recent progress in control systems, and its use is now thoroughly understood. The optimum results permitted by the nature of the physical elements can now be obtained by well-established design procedures. The relevance of these facts to the analogous problem in economics will appear later.

AN ECONOMIC SYSTEM AS A SYSTEM OF INTERDEPENDENT QUANTITIES

The economist, like the biologist, deals with systems in which the ultimate "causes" are almost infinitely complex. Actually the engineer is in a better position—though in practice rather than in principle. In his case the "law-abiding" part of phenomena is often, though not always, so predominant that he can forget the margin of randomness entirely.

The economist may however seek for relationships between quantities that are aggregates of the amounts of individual transactions, and aggregates are little affected by the randomness associated with individual decisions. It is meaningful to enquire, for example, how the aggregate savings of a community are related to variations in its total income without being involved in the particular psychological reasons why Mr. Jones of Putney is saving up to buy a grand piano. Every such economic aggregate must however be thought of as subject to some variation, regarded as random, due to the multiplicity of neglected causes of individual economic events. The kind of "laws" that are under consideration relate to the way a particular aggregate of transactions or elements is influenced, on the average or as a total, by the other quantities chosen for the scheme of analysis. Given the laws of interaction that are true "on the average" between a system of quantities, the behaviour of the system may be deduced. The actual behaviour will differ from this calculated behaviour to the extent that the random elements affect the quantities and the dependences, but the difference may be small. An efficient "scheme of explanation" is one that attributes as little as possible of the variation that occurs to hazard or accident. The real problem in this branch of economics is the selection of the simplest possible set of quantities or variables, and relations of dependence between them, which will account for the behaviour of the system most completely.

The appropriate number of such variables depends entirely on the kind of problem to be solved. For example, the American economist Leontief, in an investigation of American industry,[1] analysed a system of 600 interacting

1. W. W. Leontief, *The Structure of American Economy, 1919-1929*, Harvard University Press, 1949.

industrial groups. The analysis was only made possible by the use of modern high-speed electrical calculating machines of the digital type. Its particular purpose was to investigate the incidence of changes (such as, for example, the expansion of the aircraft industry) on these industrial groups and the subsequent repercussions due to the passing on of orders, the increase in employment and so on.

Where the interest is in the behaviour of the system "as a whole", analysis in terms of a very much smaller number of aggregates is appropriate. The transactions of large sectors of industry, or, for example, of only the two groups, "capital goods industries" and "consumer goods industries", can be lumped together and treated as single aggregates.

Recently, notable contributions by R. M. Goodwin,[1] R. Stone[2] and others, employing the mathematician's shorthand of "matrix algebra", have shown how analyses using large numbers of "quantities" are related to simpler schemes using fewer but larger aggregates. This work provides a sound basis for the proper definition and statistical measurement of the aggregates to be used in such analyses.

THE STRUCTURE OF AN ECONOMIC SYSTEM

The contribution that J. M. Keynes made to this kind of analysis in his *General Theory* lay chiefly perhaps in a brilliant choice of an extremely simple set of quantities adequate to display the essential types of behaviour of the system. In particular he made significant the distinction between the production of consumption goods and the production of capital goods.

It will be useful first to look at the scheme of dependence corresponding with Keynes' simple analysis of the conditions for equilibrium of an economic system, which he used to explain the possibility of persistent under-employment.

The scheme is shown in Fig. 5. It relates five quantities, shown by the circles, and it refers to average values over some sufficient time. The quantities are measured in money values. The circle Y represents the total of incomes, which is made up by P_c, the wages, salaries and profits disbursed in the production of consumption goods, and I, the disbursements in the making of investment or capital goods. Over the same period incomes divide between purchasing, P, and savings, S. Keynes postulated that the fraction k of incomes is spent, and he called k "the propensity to consume". Since over any sufficient time the money spent in the purchase of consumption

1. R. M. Goodwin, "The Multiplier as Matrix", *Econ. Journal*, Vol. 59, No. 236, Dec. 1949, p. 537.
2. R. Stone, "Simple Transaction Models, Information and Computing", *Rev. Econ. Studies*, Vol. 19(2), No. 49, 1951-2, p. 67.

goods is equal to their costs plus profit, i.e. to P_c, one has $P = P_c$. This relationship results in a closed loop.

Then

$$Y = kY + I$$

or

$$Y = I \frac{1}{1 - k}$$

Thus the total of incomes is determined by the rate of investment. It is in fact a simple multiple

$$\frac{1}{1 - k}$$

of the rate of investment. This factor Keynes called the "multiplier". The quantity I must in this argument include not only "investment" in the narrow sense, but all disbursements other than those entering into the prices of consumption goods.

The relationships also imply:

$$Y\ (1 - k) = I$$

But

$$Y\ (1 - k) = S \text{ by definition,}$$

which provides the proposition $S = I$, or savings equal investment, with the implication that if the rate of savings were *initially* greater than the rate of investment the level of economic activity and incomes would fall, or in the reverse case would rise, until this equality was achieved.

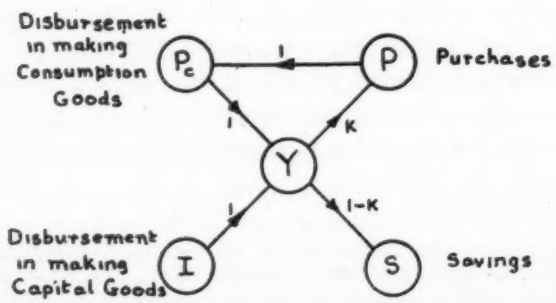

Fig. 5. Keynes' scheme for considering the conditions of equilibrium of an economic system.

This simple argument provided the essential basis for the enunciation of full-employment policies. It implies that the level of economic activity may be raised by increasing "investment" or decreasing "saving": "investment" must be understood to include government expenditure not balanced by taxation, together with the positive foreign trade balance. Measures that vary these quantities are certainly within the power of governments. Does it follow, therefore, that given the will to do so, full employment may be permanently ensured?

To draw such a conclusion from this argument alone would be foolish. The argument relates to averages over some not very clearly specified time. It establishes that the average level of economic activity can be raised, but the model considered is an "equilibrium model" and the argument so far has said nothing whatever about the possibility of fluctuations. It still remains to be considered whether, for example, a small recession might become cumulative as a major slump, or a slight upward trend might produce cumulative pressures on employment, prices and wages.

Keynes did indeed discuss these matters, but it is only during the past decade or so that appreciable progress has been made towards dealing with them in a quantitative way. This is one of the main topics of the branch of economics now known as econometrics.

ECONOMIC DYNAMICS AND SELF-EXCITING VARIATIONS

To be useful for the discussion of fluctuations and variation of the quantities, the model or scheme of interdependence must now be redrawn to represent the relationships not only on the average or in equilibrium, but also when the quantities are varying. The time-relationships must therefore be properly specified. It is also very convenient, and in most cases possible, to regard the quantities or aggregates as having two parts which may be considered

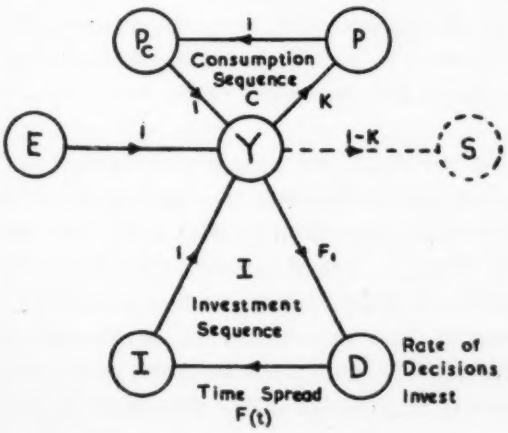

Fig. 6. Main lines of a scheme of interdependence for the discussion of the dynamic behaviour of an economic system.

E represents any supplementary expenditure regarded as independently determined.

separately, namely a set of values in equilibrium which are either constant or vary only slowly with time, and the fluctuations and oscillations about these equilibrium values. Then, to discuss the possible fluctuations, the symbols on the scheme of dependence can be taken as representing only the fluctuating part. It is understood that the values obtained have to be added to or superimposed on the equilibrium or trend values.

In drawing up the revised scheme of dependence, the chief new fact to be represented is that the rate of investment certainly varies during fluctuations, and it varies as a function of economic activity represented by aggregate income. Fig. 6 shows the general structure of a scheme of interdependence now elaborated to the minimum extent required to express these facts. A new quantity has been introduced, the "rate of decisions to invest", D. This is not the same as the actual *disbursement,* I, that constitutes the rate of investment. After any elemental decision to invest, the corresponding disbursement I follows, spread out in time, so that I must be distinguished from D and shown separately, with D producing I via a time-spread function symbolized by F(t). Similarly time-delay effects must be taken account of in several other dependences. Introducing the dependence of the rate of decisions to invest on the level of incomes Y, symbolized by F_1, makes Fig. 6 appear as two closed loops or "closed sequences of dependence", joined by the quantity Y which is common to both loops.

In considering how the system shown in Fig. 6 would behave under conditions of change, it may first be assumed that the time-lags in the consumption goods sequence (loop C) are relatively small, so that the effect of this loop is not very different from the effect that has already been considered for the case of equilibrium, namely to introduce the constant "multiplier". This makes Y greater than it would otherwise be by the factor

$$\frac{1}{1 - k}.$$

This makes it possible to represent the system even more simply, as shown in Fig. 6(a), namely by the "investment" loop only, but with the feed-back increased by the multiplier since this is the sole effect of the consumption sequence.

The remaining dependences that constitute this loop are two only. The first, symbolized by F_1, is the way the rate of decisions to invest, D, depends on variations of incomes, Y. The second, symbolized by F(t), is the way the resulting disbursement I is, on the average, spread in time subsequently to each element of the decisions to invest D. These are the central and decisive relationships in economic dynamics. The time-spread of investment is relatively straightforward and ascertainable, but the determinants of the rate of decisions to invest are less obvious and require closer investigation.

However, it is easy to see the general nature of this dependence. A good deal of discussion has been based on the following simple if somewhat inadequate view.

It may be supposed that, in the main, capital goods are required as means of production, so that when there is more production, measured by an upward change of incomes Y, more capital goods are needed. Thus the rate of decisions to invest may, as a first approximation, be assumed to be proportional to *the rate of change* of incomes, i.e. to the rate of change of Y.

If this simple assumption be made, one can see how incomes Y will vary, by following the sequence of dependences from Y round the loop back again to Y. A *rate of change* of Y at a particular time causes a proportional contribution to D, the rate of decisions to invest, and this results in Y being increased by definite additional amounts at subsequent times, as determined by F(t).

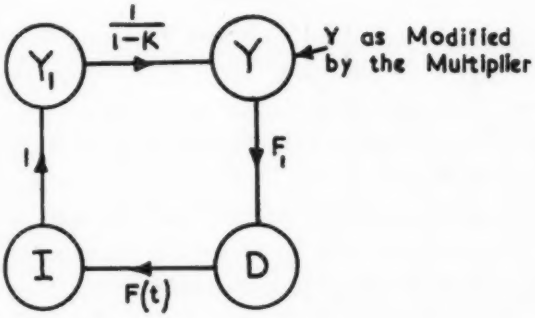

Fig. 6(a). A simple single feed-back loop representing the principal relationships in any self-sustaining oscillation.

There are two ways in which the implications of such relationships may be determined. The first is to proceed step by step to see what happens in successive small time intervals. If the course of Y over some past time is already known, it is quite easy, though rather laborious, to calculate the value of Y for the next short period, since this is made up of all the contributions arising from *past* values of the rate of change of Y, delayed by the various times.

The second method is the rather more sophisticated one of the mathematician, who enquires what type of curve, represented by what formula, satisfies these conditions.

By either approach the result is found that, if the feed-back is sufficient in magnitude, a scheme of dependence of this kind must result in an oscillation of constantly increasing amplitude.

To demonstrate this, a calculation has been made, using the time-spread function shown in Fig. 7(a) (which gives the delay in time between the decision to invest and the actual investment), and a feed-back magnitude chosen numerically as the factor 1.2. The resulting history of incomes Y is

shown in Figs. 7(b), 7(c) and 7(d). Fig. 7(b) shows what would happen if the system, initially in equilibrium, were disturbed by a temporary extra rate of investment or expenditure due to some external cause. The result is a progressively increasing oscillation which would continue indefinitely, except for the fact that checks are inevitably encountered when it becomes sufficiently large.

In Fig. 7(c) is shown what would be the effect of two kinds of check. Firstly it has been assumed that the total upward fluctuation of incomes cannot increase beyond a definite limit, on account, for example, of "full employment" being reached. As a lower limit it has been assumed that the rate of decisions to invest cannot be reduced beyond zero. It will be seen that the oscillation now settles down to a regular "limit cycle" of constant amplitude.

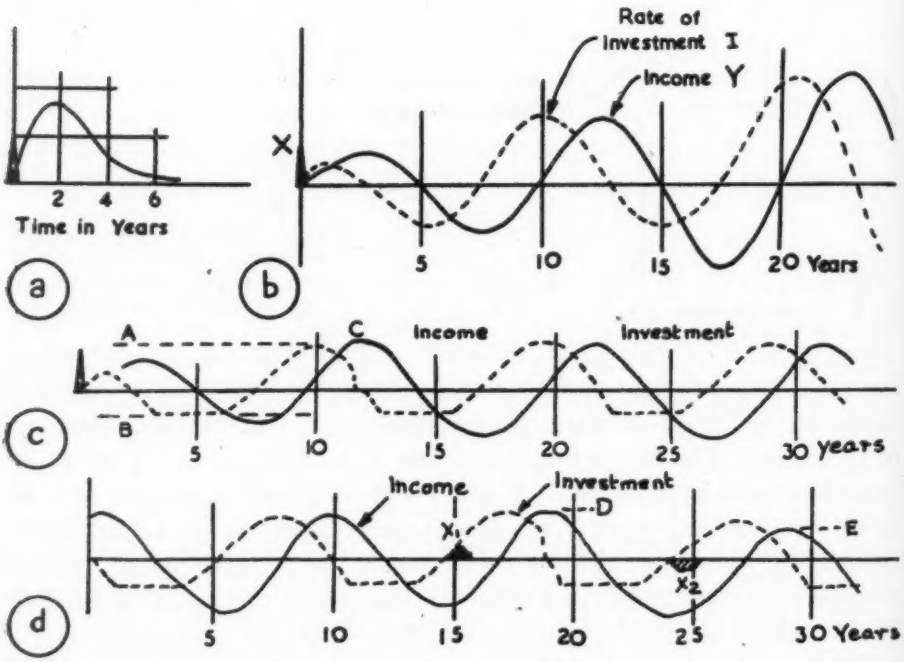

Fig. 7. Results of a calculation of behaviour of a very simple model of the kind shown by Fig. 6, showing marked resemblance to the behaviour characteristic of economic systems.

(a) Assumed time-spread function F(t).

(b) The cumulative variation from equilibrium following a small disturbance X in the absence of non-linear limiting checks.

(c) The same except that an upper limit A is imposed on income, and a lower limit B on the rate of investment, showing how a regular "limit-cycle" results. "Full employment" occurs only at C.

(d) Continuation of the limit cycle Fig. 7(c) with additional disturbances at X_1 and X_2, showing irregularity of cycle that results. Full employment is again reached at D, but the down-turn at E is far short of full employment.

In Fig. 7(d) it is supposed that some small further disturbances occur, and the effect is to make the cycle irregular. The upper and lower turning points may occur earlier or later, and the heights of booms and the depth of slumps are no longer equal.

This highly simplified "model" represents only one of the sequences (though the major one) of cause and effect that are present in the real economic system, and represents it only approximately. However, it necessarily behaves in much the same general manner as our economic system used to behave in practice. It performs oscillations having some regularity, but they are not precisely regular because there are disturbances. Each cycle has a rather sharp down-turn and a slow recovery, such as are observed in actual trade cycles. Calculations relating to the typical behaviour of simple models of this general kind, with slight variations, have been made by many economists, such as Kalecki,[1] Hicks,[2] Goodwin[3] and others.

A REALISTIC SCHEME OF INTERDEPENDENCE FOR AN ECONOMIC SYSTEM

It is not of course suggested by economists that such very simple models as the one just considered are adequate for quantitative investigation of actual economic systems. They are models crudely representing the main elements of the structure. The results that have been obtained, i.e. the similarity between the behaviour of such models and that of economic systems, give good grounds for the belief that, qualitatively or descriptively, the models do demonstrate the essential nature of the mechanism of economic instability. They would not be expected to do more. An engineer designing a stabilizer for an automatic pilot might find it most helpful to think of the behaviour of a simple pendulum, and he would then be focussing correctly on the essentials of the situation, but for detailed analyses and design of stabilizing processes and their timing a representation much more realistic in detail would be required.

Present progress in econometrics centres round the development of "models" of the economic system which include all those quantities, effects, and distinctions that detailed economic analysis suggests to be relevant to reasonable accuracy in prediction. This task presents three main aspects: the selection of the quantities and of the scheme of interdependence, the discovery of the correct nature, law or specification of the various relationships of interdependence, and the computational problems. The second of these must be given particular attention, for the point of view expressed in

1. M. Kalecki, "A Macrodynamic Theory of Business Cycles", *Econometrica*, 1935, p. 327.
2. J. R. Hicks, *A Contribution to the Theory of the Trade Cycle*, Clarendon Press, 1950.
3. R. M. Goodwin, "The Non-linear Accelerator and the Persistence of Business Cycles", *Econometrica*, Vol. 19(1), Jan. 1951, pp. 1-17.

this paper meets with two objections from certain economists. The first is that relationships of dependence of sufficient constancy or uniformity do not exist, the second is that if they do exist, it is impracticable to discover them in the quantitative form that would be required for useful prediction. There are important elements of truth in these contentions, and they must be closely examined.

A number of the relationships in a scheme of dependence can be excluded immediately from such doubts, namely those that are mere expressions of the definitions of quantities or that arise from the conventions of accountancy.

The relationships significantly in question are those that involve human behaviour. It has already been explained that the complex determinants of the different actions of each individual are not in question. These almost infinitely varied causes contribute an element of randomness that is always present, but that becomes small in relative significance if the numbers of individuals are large.

To take a simple example of a definitional relationship, the money value of purchases of consumption goods, say P, is related to incomes Y and savings S by

$$S = Y - P.$$

This may be taken as the *definition* of S, and is not open to question. Keynes' analysis for equilibrium considered the implications of the hypothesis that k, the propensity to consume, was constant over a long period. This was indisputably a useful thing to do, and in doing so, Keynes enunciated no "economic law". He did not assert that the "propensity to consume" had been constant over some particular period, or would have some particular value in future. Keynes' analysis of this hypothetical situation is in any case helpful to understanding economic events in so far as it provides a new bit of mental mechanism. One can now begin to visualize economic processes, and to have at least some clue as to what consequences would result if k were in fact constant or if it varied. One can get no further than this without requiring actual data about the particular system at the particular period. Is k in fact constant? If it is not, then on what quantities is it dependent, or to what extent is it arbitrary or random?

It is indispensable for further progress that, in the case of the "propensity to consume" and of all the other significant dependences of the system, mere plausible hypotheses should be replaced by quantitative relationships that can be presumed to hold with small random error for some period, however short, starting from the present even if the dependences found are complicated. No one supposes, nor does progress require one to assume, that consumer habits obey universal laws or are the same today as, say, before the war. All that is required is that we should be able, subject to small probable error, to say that, for example, the rates of purchasing of consumption goods

over the near future will depend in definable ways upon the values and variations of other quantities such as the past and present values of total income, the price index for consumption goods, perhaps the season of the year, and the existing volume of savings, or whatever else realistic research may reveal.

There are four chief routes to discovering effective relationships such as this, or to establishing the contrary though unlikely conclusion that there are no such dependences but mainly random, sporadic and arbitrary behaviour.

The first and most important route is the method of "field studies", which in this example would be the study of family budgets and the like, and detailed enquiry, over a representative sample, of what factors actually determined the proportion of income spent.

The second is "economic theory"—in this example the doctrines of supply and demand, and the notion of schedules of consumer preferences. This is an indispensable background, but must not override the ultimate dependence on observation.

The third route is statistical analysis. If records of the variation of the relevant quantities are available, methods of analysis exist that often enable the "best-fit" scheme of *linear* dependence to be discovered; but these methods involve extremely long calculations, and are likely to give false results if the relationships are in fact "law-abiding" but non-linear.

It is probably the difficulties and statistical snares that beset this particular class of problem in statistical analyses that account for the scepticism of some economists about the possibility of determining relationships of dependence from statistical information. The problem is particularly difficult when the system has closed sequences or feeds-back. It would however be solvable if the system were linear. The real difficulty is the combination of complexity with non-linearity.

The fourth route is really an extension of the third. It consists in adjusting all the dependences, linear or non-linear, of an assumed complete model until the behaviour calculated agrees as nearly as possible with that observed.

Economic research of this kind is time-consuming and expensive. The extent to which it is being carried out at the present time is quite inadequate. If the true importance of progress in this field were realized, it would not be tolerated that the effort going into it should remain relatively trivial. The rapid progress that is being made—in spite of the small resources available—will probably soon make it possible to set up a scheme of dependence considered to be adequate for studies of practical regulation and for a useful amount of prediction. This possibility will be held up mainly for the lack of adequate detailed studies of the comparatively few key dependences that must definitively be derived from observation.

Already, studies have been made of the system behaviour resulting from quite elaborate schemes of dependence. For example, Colin Clark made a

study of the American economy, for the period 1921-41,[1] using a model of nine interdependent quantities, and L. R. Klein[2] studied the same period with a model using 16 interrelated quantities.

It was found by these economists that the behaviour of the economy during the period considered could be accounted for to a very remarkable extent by the inherent behaviour of the assumed models, with the relations of dependence adjusted to give the best fit. The relationships so found have proved, as would be expected, to be inadequate for direct use for forecasts relating to the post-war period, partly because of the great changes due to the war and to the adoption of full employment policies, and partly because the models were inadequate, and, in the absence of direct economic data as to the dependences, too much reliance had to be placed on "best-fit" methods.

THE REQUIREMENTS FOR FURTHER PROGRESS. THE COMPUTATIONAL DIFFICULTIES

A scheme of dependence adequately representing a contemporary system must necessarily be complicated and involve a considerable number of distinct quantities. It must necessarily be non-linear, especially if it is a scheme representing a contemporary economy working in the neighbourhood of full employment, because the response to stimulus to more production encounters the ceiling set by the limited labour available. A second important non-linearity exists, in that, when a period of investment activity is followed by, say, a slight recession, there tends to be a disproportionately great drop in the rate of investment, simply because all the promising opportunities for investment have been taken up, and it takes time for new opportunities arising from obsolescence, the progress of invention and so on, to materialize.

The kind of calculations that economists wish to make about the behaviour of such a model in relation to a contemporary economic system are of two kinds.

The first kind has the purpose of adjusting the relationships, especially certain constants, so that the behaviour of the model corresponds with that of the recent past of the actual system. It is essential that as much of the structure of relationships as possible be obtained by independent direct enquiry. The final fit must then be obtained by judicious adjustment of the assumptions, giving proper weight to the degree of certainty justified on other grounds.

The second kind required is the calculation of future behaviour, which

1. C. Clark, "A System of Equations explaining the U.S. Trade Cycle, 1921-41", *Econometrica*, Vol. 17, 1949, p. 93.
2. L. R. Klein, *Economic Fluctuations in the United States, 1921-41*, Chapman and Hall, 1950.

means extending or extrapolating the behaviour of the recent past into the future.

There are major difficulties in the way of both of these types of calculation, especially the first, when, as is undoubtedly the case, a significant part is played by relationships that are non-linear. If the system were wholly linear there would be little difficulty. The behaviour of even very complex linear systems is open to analysis by several mathematical methods. There are no corresponding methods for non-linear systems. It is a weakness of some current work in this field that a solution has sometimes been sought by attempts to find an approximately equivalent linear scheme. This may lead to gross error.

There is therefore something like a computional barrier to further progress. The remainder of this article will mainly be devoted to exploring the suggestion that this barrier may be pierced with the help of mechanical aids to computation, especially by the use of "analogue computors". There is, of course, the parallel possibility of the greater use of the modern high-speed electronic *digital* computors. These machines will facilitate those operations that can be reduced to prescribed routines of arithmetic. The calculation of the behaviour of complex *non-linear* systems using digital (i.e. arithmetical) machines involves a very formidable problem of programming (i.e. representing the detailed instructions to the machine on punched cards). For this particular task the analogue type of computor rather than the digital type seems particularly suited, because of the directness with which hypotheses and values can be varied, and results interpreted and automatically plotted as graphs.

THE POSSIBLE USE OF PHYSICAL ANALOGUES

Extensive use is now being made, in the development of automatic controls in engineering (such as the automatic piloting of aircraft), of physical models that can be adjusted to simulate the system to be studied, that is, to have the same scheme of interdependence. There is no obstacle in principle to constructing physical systems that correspond, in their structure of interdependences, with the kind of economic model that economists wish to investigate, including the correct representation of the non-linear relationships, but with a time scale such that minutes or seconds represent years. If such a physical analogue were available, provided with appropriate means for varying the inter-relationships in a systematic way and recording the behaviour by curves drawn on a chart, it would be possible by systematic trial to solve the problem of adjustment of the scheme to correspond best with the economic record of a recent past. As such a machine could plot a 10-year period in a fraction of a minute, and at the same time record the

average error, procedure by trial is not impossible. The analogue, when so adjusted and fed with quantities simulating any actual or anticipated disturbances, would then immediately give the required extrapolation into future time, by merely allowing its operation to continue.

There are many types of physical analogue, depending, among other things, on the kind of physical quantity selected, e.g. whether mechanical, electric, hydraulic and so on. There are also important differences in fundamental type. One such important distinction lies in the extent of the provision made for controlling the passage of "time" in the simulator. The most commonly employed analogue or simulator uses thermionic amplifiers and electrical networks to simulate the interdependences. The quantities are simulated by electrical voltages. This type, the "electronic analogue", is unsuitable for the requirements of economists, because it has a fixed time scale. One cannot "make time stand still". There is a fixed ratio. So many seconds represent so many years.

The economist requires a simulator in which the passage of time is separately and independently simulated—according to the rotation of a shaft, for example—and is therefore under control. Then, given initial values of the quantities, or recent past values, these may be set up on the analogue with the time shaft stationary. On rotating the time shaft the machine progresses. On such an analogue the simulated progress of time may be stopped at any stage to introduce changes or adjustments of conditions. This facility seems indispensable in economic work, but it involves a much more elaborate and expensive arrangement than the more familiar electronic analogue which does not provide this facility.

One existing form of analogue calculator that in principle meets this requirement of independent control of simulated time is the well-known differential analyser. Here the quantities are simulated mechanically (by angles of rotation of shafts) and the interdependences are built up by variable speed gears, ordinary and differential gear trains, and so on.

These machines are rather cumbersome, too slow both in setting up and reading for convenience in economic work, and the existing examples of such machines are too restricted in the number of quantities they can simultaneously handle and record. It is evident that a specially designed installation will be required if the possibilities of analogue computation are to be exploited in economic research. The final design of such a computor would require a good deal of time and effort by engineers and economists working in collaboration to ensure that proper provision was made to handle the problems as they would probably present themselves, and to specify the requirements in respect of accuracy and speed, recording facilities, supplementary computational aids and so on.

The form of such an installation which the writer would foresee as the probable outcome of such an investigation, would be one in which the various

quantities were simulated by electrical voltages corresponding with the position of sliders on potentiometers, and recorded simultaneously on a multi-pen moving chart recorder. Electrical as distinct from mechanical simulation permits a much higher speed of operation.

The interdependence would be set up via interchangeable electrical units, at a central connecting-board, with suitable plugs or switches, which would permit rapid setting up and control, and flexibility of arrangement.

The basic relationships of the system to be simulated are, in the case of many dependences, merely addition, subtraction and multiplication by constants. All such dependences are very easily and accurately simulated by suitable electronic valve circuits.

A further large group of dependences required are specified time-relationships. One such time-relationship has already been discussed, namely the characteristic spread of the rates of investment over time that follows an elemental "decision to invest" for a given class of investment goods. There are many other time-delays and time-functions to be taken into account. The simulation of such time-dependences is one of the requirements that is difficult to meet while at the same time providing an independent control of simulated time. The electrical networks that so readily provide time-delay functions in the usual electronic simulator are of no use except on a fixed time-scale. The requirement can be met, but the principles involve technicalities relating to the temporary recording of electrical values that will not be described in detail here.

There are also relationships specified by differential equations, and to simulate them the electrical version of the elements of a differential analyser is the appropriate device. Readers who are familiar with electrical devices will appreciate immediately that the arrangement shown in Fig. 8, for example, simulates integration, with the time-scale independently controllable, and given an integrating element, all relationships representable by

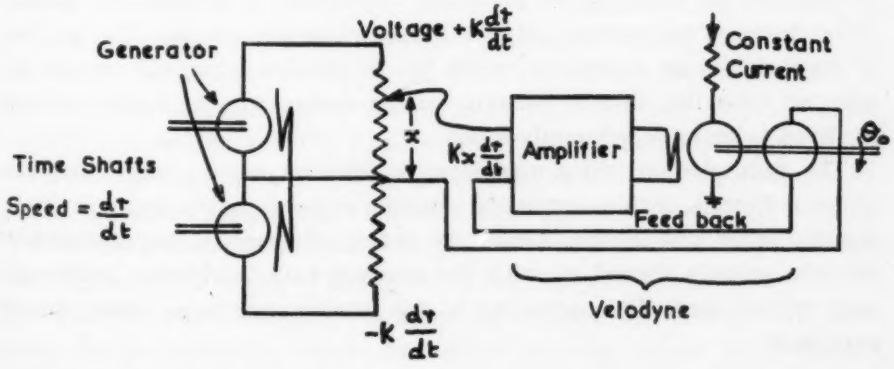

Fig. 8. A typical electrical arrangement for integration. The shaft marked Θ_0 rotates through the angle $K\int x dt$.

linear differential equations can be simulated on the same lines as in a mechanical differential analyser.

It remains to provide for the non-linear relationships. Some of these are of the kind that can be represented by a curved graph. Such graphs previously drawn on paper may be followed by an "electric eye" or photo-cell-operated curve follower, and the plotted non-linear function fed into the analogue. There are some rather more complicated types of non-linearity, but examination shows that there would be no difficulty in providing for them.

The general conclusion, which cannot be elaborated here, is that a physical analogue adequate for research into the behaviour of economic systems is technically feasible, but it would have to be one built specially for the purpose, and would require a considerable number of elements. It might cost some tens of thousands of pounds, and take several years to design, build and carry through the experimental stage.

WHAT COULD BE ACHIEVED WITH AN ANALOGUE COMPUTOR?

It must again be emphasized that in expressing the opinion that the construction of an analogue computor for economic research is worth serious consideration, the writer is not suggesting that it would, alone, be of much value. It is one desirable tool, but its indispensable complement is an intensification of field studies and statistical research to establish *independently* as many of the principal dependences of the system as possible, so as to be able to discover the others, and to make final adjustments of values by trial, using the analogue. Such an analogue computer would be of immense value in econometric research.

Assuming this stage to be successfully achieved, which will require years of work for a considerable team of economists, statisticians and engineers, there would be a substantial basis for the guidance of policy. It would then be possible, for example, for stabilizing operations by government agencies to be designed and carried out as a matter of regular routine. The purpose of such stabilizing operations would be, in the first place, to remove the inherent instability that is a feature of the system, and to ensure that the modified system was inherently stable.

The principles on which automatic stabilization can be introduced into physical systems, such as automatic controls, are now well understood. There are two main possibilities. Either the nature of some of the dependences must be suitably altered, or what the engineer calls "stabilizing feed-back" must be provided. The equivalent of this in economic terms must now be explained.

AUTOMATIC STABILIZATION OF AN ECONOMIC SYSTEM

It is evident that within the existing powers of governments there are a variety of possible actions that affect the behaviour of the economic system. Some of the familiar ones are budgetary unbalance, variation of bank rate, open market buying and selling, variation of the rate of government spending, and public works policies. There may be added such devices as purchase and sale of ballast stocks of storable commodities, variation of purchase taxes, accumulation and release of credits (on the lines of the British post-war credits), measures of social security that affect saving, programmes of technical education and research that affect the flow of opportunities to invest, and many others. Of these some evoke a much quicker response than others. The quick-acting operations are to be preferred for devising stabilizing routines.

An economic system is subject to various disturbances, due to external events, varying harvests, political changes, new inventions and so on. It is not possible, nor is it necessary, entirely to remove the minor fluctuations that directly result from the impact of these disturbances. It is certainly necessary to offset any prolonged or major or permanent disturbance, but the requirement now under consideration is that a small temporary disturbance should not result in a cumulative "explosive" or oscillatory further change.

It was explained earlier, in considering the example of the automatic pilot of an aircraft, how it was necessary that any small departure from the required course should bring about corrective action in a prescribed manner, timed so that the aircraft would turn back quickly, but without overshoot. The movement of the rudder required to bring this about is not simply a movement proportional to the error. It could better be described as a particular kind of wobble of the rudder, which at the same time corrects the error *and* damps out the oscillation that would otherwise follow. The way this is contrived is to pick up the variation of some quantity or quantities in the system that provide a suitable "index" or cue to the momentary variations, and apply an extra corrective stimulus which is in predetermined dependence on this index or cue. This is known as "adding a stabilizing feed-back". It is possible to use various quantities as cues, and various other quantities as points of correction, but naturally some combinations require simpler time-functions than others.

In the economic system, since human operation intervenes, one has a free hand in respect of timing of corrective action except of course that it cannot *anticipate* its cue. If certain quantities are selected as "indices" for automatic routine stabilizing action, one would naturally choose those that are most immediately responsible to incipient disturbance, such as, for example, the price index of consumption goods or the rate of decisions to invest.

Thus, a stabilizing feed-back in economics takes the form of *routine* operations in dependence on a certain index derived from current statistical movements, according to a fixed formula. There are simple procedures to devise the "formula" for maximum effectiveness, once the scheme of dependence of the system is known and the index and the operational means have been selected.

A surprising feature of such stabilizing devices in mechanisms is that, when they are adequate and automatic, they give the appearance of being very little used. They "nip in the bud" all tendency to oscillate, and once stability is attained no oscillation ever builds up, and the stabilizing device maintains stability with only slight further action. This might need to be qualified in respect of economic stabilization undertaken by one national authority only, because the periodicities of national systems tend to be similar, and such a national economy might find its regulating routine working hard if, at about its own natural periodicity, there arose a disturbance due to fluctuations of its terms of trade with its neighbours. This possible difficulty would disappear in so far as all major governments adopted adequate stabilizing routines.

NAUTICAL SCIENCE AND THE GEOGRAPHICAL REVOLUTION

by

ARMANDO CORTESAO

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I

One of the most remarkable examples of the interrelation between economic and social conditions on the one hand, and scientific progress on the other, is provided by nautical science, i.e. the application of astronomical, magnetic, cartographical, meteorological and hydrographical knowledge to navigation.

Several ancient peoples—the Chinese, the Indians, the Malaysians, the inhabitants of the Eastern Mediterranean and the Arabs—developed considerable maritime activities. We have only vague references to the earliest sea navigation, but it seems that in about 3000 B.C. the Babylonians went to India by sea, and that before the second millenium B.C. the Phoenicians were already sailing in the Atlantic. We do not know if or when they evolved any method or methods of finding their course when out of sight of land. However, the practice of long and distant voyages on the high seas, for centuries on end, would have favoured the working out of at least some elementary method of orientation and determination of latitude by rudimentary astronomical observations.

After the destruction of the naval power of the Phoenicians, Greeks, and Carthaginians, the Romans became masters of the sea, but they made no progress in the art of navigation. When Rome fell, the Arabs, after occupying the whole of the Iberian Peninsula, developed considerable seamanship in the Mediterranean and even in the Atlantic. They made remarkable contributions to the art and science of navigation by introducing the lateen sail in the West and by developing their already considerable knowledge of astronomy. The Vikings may have gone to Greenland and north-eastern America in the tenth century, but their great seamanship brought no actual contribution to the art of navigation, or if it did it was confined to themselves. In fact, it was not until the thirteenth century that one can see the beginnings of a real science of navigation.

Several factors contributed to this development. Under the impact of the revival of learning,[1] the progress made in mathematics and science began to yield fruit in the form of practical scientific discoveries and technological inventions. The Crusades, which extended from the end of the eleventh to the end of the thirteenth century, aroused a new interest in the outside world and certainly developed the spirit of overseas expeditions. Lastly, the idea of taking risks in fields other than war was encouraged by the newly-born capitalist system—the so-called "early capitalism" which began in the thirteenth century under the influence of the Church. "Apart from the great textile industries of the thirteenth century and the mining enterprises of the fifteenth, the chief form of capital investment was in ships and cargoes," says Lewis Mumford. The recovery and development of the great works of classical antiquity, particularly those of Ptolemy in mathematics, astronomy and geography, first by the Moslems, and then by the Jews and Christians, prepared the ground for one of the most momentous discoveries to be made two centuries later—the scientific method of determining the position of a ship on the high seas by the application of existing astronomical knowledge.

From our point of view, the most important aspect of the progress made was that the great astronomical and geographical works of Arab scholars were made known to the Latin world by John Holywood, the famous Sacrobosca (d. *c.* 1250), in his *Sphaera Mundi* which was translated into many languages and had a tremendous influence until the sixteenth century. One of these Arab scholars, the eleventh-century astronomer Al-Zagarli, not only invented an improved flat astrolabe, but also edited the so-called Toledan planetary tables.

No less noteworthy, though not so widely known outside the Iberian Peninsula, was the compilation of the *Libros del Saber de Astronomia* by the order and under the supervision of Alphonso X el Sabio (d. 1284), whom Sarton considers as "one of the greatest intermediaries between Arabic and European knowledge". This enormous work contains the translation into Spanish, by a group of Jewish and Christian scholars whom Alfonso X had gathered round him, of all known writings on astronomical matters, beginning

1. This revival of arts and letters began with Charlemagne (d. 814). The foundation of the Carolingian and cathedral schools had a profound influence on the intellectual movement which was to flourish a few centuries later. With the political unification of the Arab world a new and momentous element was introduced into the cultural evolution of Europe. Many Greek works were translated into Arabic, and, through the Persians, the Arabs assimilated from India much of their mathematical knowledge. The first universities began to appear in Europe, as developments of, or successors to, the Carolingian and cathedral schools: Bologna, Paris and Oxford in the twelfth century, followed by 10 more, Padua, Naples, Cambridge, Toulouse, Salamanca, Genoa, Perugia, Coimbra, Montpellier and Lerida, in the thirteenth. Many encyclopaedic works had already been written and were widely known. All this contributed to the start of the great intellectual and cultural stirring which was marked by a revival of Aristotelianism and the foundation of Franciscanism (its love of nature fitted in with the new approach brought to science by Aristotelian thought), and last but not least by a new methodology in mathematics. The latter was chiefly due to Abraham Savasorda, a Spanish Jew who lived in Barcelona and whose work was followed a century later by the better known Leonardus Pisanus or Leonardo Fibonacci (d. *c.* 1243).

with Ptolemy and including those of the Arabic authors as well as some original works. In this 18 *Libros* could be found not only all the contemporary theoretical knowledge on astronomy, but also the description of such instruments as astrolabes, quadrants, armils and clocks, and practical instructions for making them. The existence of this mass of special knowledge in the Iberian Peninsula is of considerable historical importance.

II

Such was the state of knowledge in the thirteenth century, when Europe, under the spur of the newly-born capitalism, began her seafaring expeditions. In the 1270's some galleys commanded by Lancelot Malocello, member of a noble Genoese family, sailed towards the "Fortunate Islands". There a fort was built in the Canaries. In 1291 two other galleys commanded by the brothers Vivaldi sailed from Genoa to the Atlantic so "that they might go by sea to the ports of India and bring back useful articles of merchandise". The Vivaldi reached the Canaries but never returned, and several unsuccessful expeditions were sent from Genoa in search of them. Some thirty years later the Italians, Majorcans, Portuguese and French followed suit, and expeditions to the Canaries became a common occurrence. Many of these ships were provided only with sails, and since they could not sail against the north-east trades and currents, they had to put further out to sea in order to take advantage of the favourable winds which would bring them back home.

The difficulties caused by the north-east trade winds on the return journey from the Canaries and further south led to the development of the lateen-rigged caravel, which was lighter, swifter and much more manoeuvrable than the ship rigged with square sails only, and therefore could sail near the wind and was even able to tackle the head winds. It was thanks to the caravel that the Portuguese reached Cape Verde in 1443 and the Gold Coast in 1446; in 1460 they discovered the Cape Verde Islands and by 1475 had explored the African coast as far as latitude 9° S. After the lateen-rigged caravel had been increased in size from 50 to 150-200 tons, with three or four masts, square sails were added to it to help it to undertake longer voyages; on the other hand, lateen sails were added to the square sails of the bigger ships in order to allow them to take advantage of less favourable winds. Bartolomeu Dias' voyage round the Cape of Good Hope in 1487-88 was still made with four caravels of two or three masts rigged with lateen sails only; but in the three-masted caravel that took Columbus to America in 1492 the main mast was rigged with a square sail, and in the ships that took Vasco da Gama to India in 1498 the after mast was rigged with a lateen sail.

These developments in the art of navigation, designed to make longer voyages possible, were essentially the outcome of a new economic situation

which had arisen in Europe in the first half of the fifteenth century. In the late Middle Ages aromatic spices such as cloves, nutmeg, cinnamon, ginger, and particularly pepper, had become so much in demand that they were often used as currency and to pay taxes. They came to Europe from the East via the Red Sea and Egypt, or Syria, and were distributed to Western Europe through Genoa and Venice, and to Eastern Europe through Constantinople. These spices had to be paid for in gold, and their growing use, together with the development of international trade in general, imposed a severe strain on Europe's gold resources. Now much of the gold and some of the pepper came from north and north-west Africa respectively and was in the hands of the Arabs. Since the beginning of the fourteenth century the Genoese had tried to find out the routes which the Arab caravans used for bringing the gold to the southern Mediterranean ports, particularly to Ceuta, which the Portuguese had taken from the Moors in 1415. Thus were begun a series of systematic explorations of the Atlantic coast of Africa. And when the Portuguese reached Guinea they called it "Malaguetta Coast" (from the latin name of an aromatic spice), whilst the land further south they called "Gold Coast".

This did not solve the problem, however and meanwhile, after the fall of Constantinople in 1453, Egypt was left as the only commercial intermediary between East and West and greatly increased its prices. Furthermore, Arab pirates in the Mediterranean became more and more daring, and had the sea trade there practically at their mercy. It thus became imperative to find some means of bringing the spices from the East directly by sea. This naturally meant circumnavigating Africa. The idea had already been suggested by the Majorcan Ramon Lull and the Venetian Marino Sanudo at the beginning of the fourteenth century. It was now taken up by the Portuguese.

III

During the course of their early sea explorations the Portuguese became well acquainted with the system of winds and currents, and soon realized that, in order to reach India, it was necessary to cross the South Atlantic south-westwards and then sail south-eastwards round the Cape of Good Hope. (This was actually accomplished by Bartolomeu Dias in 1487.) But to sail across the South Atlantic meant high seas navigation, which can only be done with any measure of security if there is a process of finding the ship's position with some degree of accuracy and at reasonable intervals. The compass, then more than a century old, though indispensable to navigation, could not solve the problem by itself. In the late Middle Ages navigation was by dead reckoning, i.e. the ship's position was estimated from the distance run and the course steered by the compass. Though some good

results could be obtained occasionally, the insufficiently accurate reckoning of the distance run and the impossibility of estimating corrections for currents, leeway, etc. generally rendered high seas navigation impracticable. The only way in which a ship could find her position out of sight of land was by determining its latitude, which in turn could be done by calculating the meridian altitude of some heavenly body. Once a method for performing this simple but essential operation had been found and developed, nautical astronomy—the basis of high seas navigation and the foundation of modern geography—was created.

The possibility of finding latitudes was known in ancient times, perhaps as early as the second millenium B.C. or even earlier. Through the Arabs, the use of the astrolabe and the quadrant was current in the Iberian Peninsula. The astrolabe was a complicated instrument used mostly for astrological operations, and frequently there was even a circular calendar scale, a circular rotating star-map and a plate with a polar projection of the circles of altitude of the celestial sphere engraved on it. It is only natural that the most active navigators of the first half of the fifteenth century, faced with the urgent need for an accurate enough process of finding the ship's position on the high seas, thought of using the astrolabe and quadrant for determining the latitude by observations of the pole-star and of the sun, already practised on land for some centuries. Thus, with a positive element—the latitude of the place—the dead reckoning could be corrected and the ship's position pricked on the chart with a near enough approximation.

The first thing to be done to make the use of the astrolabe practical on board was to strip it of its astronomical complexities. This was accomplished by reducing it to a simple flat ring with two of its quadrants graduated in degrees, at the centre of which pivoted an arm with two pinhole sights. At the same time the diameter of the ring was enlarged in order to give more accuracy to the observations. So the astronomical astrolabe became the nautical astrolabe. The astrolabe used on board was about thirty centimetres in diameter; but it could be much larger for observations ashore and give more accurate results than would be possible on board due to the motion of the ship. When Vasco da Gama went to India for the first time he used a large astrolabe of wood, about seventy centimetres in diameter, suspended from a tripod, with which he made observations ashore. Though the first recorded actual observation of astronomical latitudes at sea dates from 1462, it is likely that they were already made some ten or more years earlier.

This progress in navigation was matched by progress in cartography. Among the learned men assembled around Henry the Navigator was the Majorcan Jew Jafuda Cresques, an expert cartographer and instrument maker, who came to Portugal some time between 1420 and 1427. Jafuda was the son of Abraham Cresques, maker of the famous map of the world (extant in the Bibliothèque Nationale of Paris), and he probably helped his

father to draw this map between 1375 and 1381. We do not know how long he lived in Portugal, but his influence was certainly great, not only as a cosmographer and maker of the first instruments, but above all as a maker of nautical charts and maps, an art which he taught to his pupils. Azurara, Prince Henry's chronicler, refers to the detailed "navigation charts which the Infant caused to be prepared" of the newly discovered coasts. This early cartography has been called by Beazley and Prestage "the first hydrographical maps of the west coast of Africa" which "were adopted and copied by the cosmographers of the whole of Europe". These fifteenth-century charts do not differ much from those used in the Mediterranean, probably for some centuries, though the earliest extant specimen dates from *c.*1300. They were "compass charts", with a system of wind-roses forming a network of rhumb-lines,[1] which helped the pilots to direct their course by the compass and the "Toleta" or "Raxon de marteloio"—a table of figures, probably of Italian origin, corresponding to different rhumb angles, which enabled the pilots to estimate the ship's position after having changed her course.

While sailing north of the equator, no difficulty was encountered in the operation of finding the latitude, either by the height of the pole-star, which was very simple, or of the sun at noon. For the latter purpose there were already even before the fifteenth century several treatises on the astrolabe and on the quadrant, as well as instructions and tables of declination or "shades". But these tables of shades were limited to the northern hemisphere, and as the ships approached the equator, which was actually crossed in 1471, observation of the pole-star became impossible. Later on rules were worked out for determining the latitude in the southern hemisphere by observations of the Southern Cross. Meanwhile it became imperative to prepare new tables of declination for determining the latitude of any place by its relation to the sun's noon altitude and its declination, i.e. its position in the Zodiac.

The problem was solved by King John II's so-called Junta of astronomers and mathematicians, by adapting and applying to the southern hemisphere the method used for the northern hemisphere. John II, "the Perfect", perhaps the greatest Portuguese of all times, succeeded Henry the Navigator, his great-uncle, in the leadership of the discoveries, and it was he who directed the vast preparatory work and the organization of Gama's expedition to India. The chief scientist in the Junta was the Portuguese Jew, José Vizinho, a disciple of Abraham Zacuto. Zacuto, professor of astronomy at the University of Salamanca, had composed in 1473-78 an *Almanach Perpetuum* in Hebrew. This, which has rightly been called "the immortal monument of fifteenth-century astronomy", and its author had a great influence on the

1. The compass is divided into 32 segments of 11°15' each, called rhumbs, which together form the wind-rose.

development of nautical science, and Zacuto's *Almanach* was probably used by Vizinho when he was sent to the coasts of Guinea in 1485 in order to try out the new tables on board ship.

The tables of declination were first calculated for a single year; then four-year tables were prepared, and the latter were already used in Gama's voyage to India in 1497-98. As the first tables prepared were too complicated for the sailors' rudimentary culture and needs, they were simplified and assembled, with some useful data and detailed instructions, in a practical manual known as *Rule of the astrolabe and of the quadrant*. Though often copied by hand, these manuals, or *Regimentos,* were soon printed. The earliest copy extant, printed perhaps as early as 1495, is divided into two parts. Part I contains: (a) detailed instructions for calculating the latitude from the sun's meridian altitude, with several examples according to whether the observer is on the equator, or north or south of it; (b) rules for finding the latitude by the height of the pole-star; (c) a list of latitudes of 60 sea coast places north of the equator (the next edition known gave the latitudes of 193 places north and south of the equator); (d) rules for plotting the ship's track; (e) calendar and tables of sun's declination for 12 months. Part II contained only a Portuguese translation of Sacrobosco's *Sphaera.* Later editions showed considerable improvements. The *Regimentos,* with some adaptations or additions, were soon translated into Spanish, English, French, Italian and other languages.

John II's Junta brought the preparatory work to an end: it was now possible to determine the latitude of any position at sea by observing the altitude of a heavenly body. Modern nautical science was born.

IV

Not only astronomical navigation but also other branches of nautical science, such as the system of Atlantic currents and winds, and the computation of the ebb and flow of the tides, were carefully studied during the fifteenth century. Magnetic declination, which was known at least as early as the thirteenth century, was observed by Bartholomeu Dias well before Columbus, and then studied scientifically for the first time by D. Joao de Castro. The latter realized that magnetic deviation was a variable quantity which had no connexion with differences of meridian, though cosmographers still sought for many years to solve the problem of longitudes by the compass. New instruments were invented and methods devised for calculating the latitude at sea by astronomical observations at any time of the day or night. In cartography, Pedro Nunes realized that when a ship follows a fixed course she does not describe a straight line, but a curve which cuts successive meridians at the same angle, i.e. a loxodrome. This peculiarly irregular curve

cannot be traced as a straight line on a flat chart, except when the rhumb followed is dead north-south or east-west. He published his study and definition of the loxodrome in 1537. But it was only in 1569 that the great Flemish cosmographer Gerard Mercator gave graphic expression to the study of the loxodrome in the famous projection which is linked with his name, and which, after being further studied and improved by the English mathematician Edward Wright in 1599, is still today the most useful map of the world in the practice of navigation. But the vexed, though secondary, problem of determining the longitude at sea could not be solved satisfactorily until Harrison's invention of the marine chronometer in 1735, which had been preceded five years earlier by that other very important English invention, the sextant.

Thus, the three greatest steps in the development of nautical science were: (a) the discovery of nautical astronomy; (b) the invention of Mercator's projection; and (c) the invention of the marine chronometer. With the latter two steps, navigation was raised to an almost exact science—even though wireless and radar were not to come until two centuries later. The main problem had, however, been satisfactorily solved in the fifteenth century; so satisfactorily, indeed, that before it ended, America and India had been reached by sea and a new era began for mankind.

It was the intellectual, economic, social and political revolution through which Europe had passed during the two previous centuries that imposed the maritime expansion which, in its turn, was rendered possible by the development of nautical science. And, though the Portuguese were the first to discover modern astronomical navigation, it was the result of the accumulated experience and efforts of many peoples throughout the centuries.

BOOKS AND PUBLICATIONS

The Scientistic Attitude

F. A. HAYEK, *The Counter-Revolution of Science. Studies on the Abuse of Reason,* The Free Press, Glencoe, Illinois, 1952 (London distributors: Allen and Unwin), 255 pp., $4 or 32s.

Henri Comte de Saint-Simon (1760-1825). Selected Writings, edited and translated with an introduction by F. M. H. Markham, "Blackwell's Political Texts", Basil Blackwell, Oxford, 1952, li + 116 pp., 12s. 6d.

When, in 1941, Professor Hayek published in *Economica* the now famous series of articles on "The Counter-Revolution of Science", it was at once apparent that here was one of the most brilliant and significant essays on the intellectual history of totalitarianism. Not long afterwards, in the issues of *Economica* of 1942-44, appeared another series of studies from Professor Hayek's pen on "Scientism and the Study of Society". Finally, after having left the chair of economic science and statistics at the University of London for that of social and moral science at the University of Chicago, Professor Hayek contributed to *Measure* of June 1951 another essay, written originally as a lecture, entitled "Comte and Hegel". These three essays have now been amalgamated, with minor revisions, into a single volume, in a logical order rather than that in which they appeared: the theoretical discussion of scientism comes first, followed by an historical account of the development of the ideas in question, and then by the rather unorthodox interpretation of the common influence of Comte and Hegel. The result is a fully unified book which is perhaps the most penetrating study that has yet appeared of scientism and its far-reaching influence on political and social thought.

Scientism, briefly defined, is the illegitimate extension to the study of society of the methods which have succeeded so well in the natural sciences.

> . . . we shall, wherever we are concerned, not with the general spirit of disinterested inquiry but with slavish imitation of the method and language of Science, speak of "scientism" or the "scientistic" prejudice . . . in the sense in which we shall use these terms, they describe, of course, an attitude which is decidedly unscientific in the true sense of the word, since it involves a mechanical and uncritical application of habits of thought to fields different from those in which they have been formed. The scientistic as distinguished from the scientific view is not an unprejudiced but a very prejudiced approach which, before it has considered its subject, claims to know what is the most appropriate way of investigating it.

This attitude towards the study of social and economic phenomena first began to assert itself in the early part of the nineteenth century. The success of the methods employed in the natural sciences was such that scientists and those fascinated by science tried to force them on the social sciences. The phenomenon of men tending to overstrain a new principle of explanation is not uncommon: gravitation, evolution, relativity, psycho-analysis have all

been strained at some time far beyond their capacity; but in this case it was science as a whole, and not just a particular scientific doctrine, which was overstrained beyond the limits of its validity. The chief fallacies of this scientistic reasoning are analysed by Professor Hayek under the three main headings of objectivism, collectivism, and historicism.

Objectivism, in this sense, is the attempt to dispense with our subjective knowledge of the working of the human mind. But so far as human actions are concerned, things *are* what the people acting *think* they are. The "facts" of the social sciences are also opinions, ideas or concepts, not the opinions of those who study them, but the opinions of those whose actions produce the object of the study. The social sciences deal with phenomena which can be understood only because the object of our study has a mind of a structure similar to our own. Any knowledge which we may happen to possess, but which the people whose action we want to explain do not possess, is as little relevant to the explanation of their actions as our disbelief in the efficacy of a magic charm will help us to understand the behaviour of the savage who believes in it.

This objectivism also finds expression in a blind striving for quantitative measurements which, in Professor Hayek's opinion, is responsible for the worst aberrations and absurdities produced by scientism in the study of society. It often not only leads to the selection of the most irrelevant aspects of social phenomena simply because they happen to be measurable, but also to measurements and assignments of numerical values which are absolutely meaningless—"of the same logical type as Plato's determination that a just ruler is 729 times as happy as an unjust one".

Collectivism is the tendency to treat wholes like "society", "capitalism", "class", etc. as definitely given objects about which, on the analogy of a biological organism, we can discover laws by observing their behaviour as wholes. Natural scientists are accustomed to seek first for empirical regularities in relatively complex phenomena that are given to observation, and then try to explain them in terms of simpler elements. But the wholes of social science are never given to our observation; they are without exception constructions of our mind and cannot be viewed "objectively" as something of which the human mind is not a part. We can only study them through the subjectivist approach, from our knowledge of the inside of these social complexes, and of the individual attitudes which form their structure.

Finally, historicism in its modern meaning is the attempt to make history a science, the only science of social phenomena. It affirms that history is the only road which can lead to such a science, that it is the empirical material from which laws similar to those of physics and other natural sciences can be drawn. But in social phenomena the explanation of particular and unique situations is often more important and of greater interest than any generalization because, like the rise and fall of the Roman Empire, or the French Revolution, or the industrial revolution, they have contributed decisively to the creating of the particular environment in which we live. These attempts at a philosophy of history endeavour to find laws where, in the nature of the case, they cannot be found. But from Hegel, Comte, and particularly Marx, down to Sombart and Spengler, this belief has been so widely held that it has exerted a profound influence on social evolution—mainly because it looked as though the laws of history were of the same kind as those which are valid in the natural sciences.

These three fallacies are, in Professor Hayek's opinion, the common denominator of the countless attempts to create a real science of society which have been made in the last four generations. The great differences between the methods of the natural sciences and those of the social sciences explain why the natural scientist so often feels that social scientists commit all the mortal sins which he is most careful to avoid. From this belief to the attempt to create a real science of society, a kind of "social physics" or "social energetics", is only one step—a step which has been blithely taken many a time in the last 120 years by someone who has hoped thereby to revolutionize the study of society.

But the practical consequences of this scientistic attitude reach far beyond these abortive and relatively harmless attempts at creating a real science of society. From a pragmatic interpretation of social institutions, which holds that nothing that has not been consciously designed can be useful, and denies the possibility of an orderly or purposeful arrangement in anything which is not the result of deliberate design, it is an easy transition to the belief that we must have complete power to refashion social institutions in any way we desire. This transition is made the easier by the belief that, just as the engineer has complete knowledge and control of the materials with which he builds his structures, so the "political engineer" or the "social engineer" (Lenin called artists "engineers of the soul") can have the complete knowledge and control of the materials with which to build his political or social structures.

"Social" or "political engineering", however, are only peculiar manifestations of a much wider trend which can be traced directly to scientism. This is the universal demand for conscious control or direction of social processes, which finds its most popular expression in the demand for "economic planning" and which, perhaps better than any other of its clichés, expresses the peculiar spirit of our age. This political collectivism is the natural consequence of methodological collectivism which, in its turn, is the direct product of scientism. Indeed, without methodological collectivism, political collectivism would be deprived of its intellectual basis. The majority of the intellectual leaders of socialism are socialists because socialism appears to them as "science applied in clear awareness and with full insight to all fields of human activity".

Not only was this intellectual trend "the counter-revolution of science" (as the Napoleonic reactionary Bonald called it), but it was also, above all, a counter-revolution of scientists, as Professor Hayek demonstrates in the fascinating historical account from which the book takes its title. Most of the leading ideas of French positivism originated with d'Alembert, Turgot, Lagrange and Condorcet, three of whom were scientists. And the nutritive milieu in which these ideas grew and matured and from which they spread through society, was the body of professional scientists and engineers formed at the newly-created institution which embodied the spirit of the age as did no other, the Ecole Polytechnique at Paris. It symbolized the entirely new educational system brought about by the revolution, the concentration on scientific subjects to the almost complete exclusion of classical education:

> For the first time in history that new type appeared which as the product of the German *Realschule* and of similar institutions was to become so important and influential in the later nineteenth and the twentieth century: the technical specialist who was regarded as educated because he had passed through difficult schools but who

had little or no knowledge of society, its life, growth, problems and its values, which only the study of history, literature and the languages can give.

Here Professor Hayek seems to suggest that some of the defects so characteristic of German education had their roots in the French post-revolutionary system.

In fact, nearly all the leading members of the Saint-Simonian group, including the greatest of them all, Comte, were products of the Ecole Polytechnique. Oddly enough, the founder of the movement himself was not, though even he, when he decided to embark on a new science of society, found it natural to take up his abode opposite the Ecole.

The very fact that the first English translation of selected writings of Saint-Simon has appeared 127 years after his death is perhaps as good a measure as any of the greatness of the man, if by greatness we mean the uncanny ability to foresee the history of the next hundred years. For if there is one epithet that fits Saint-Simon, it is that of precursor. He was the precursor of socialism, of totalitarianism, of technocracy; and although the term "sociology" was invented by his pupil Comte, the idea came from him and was in fact the essence of his philosophy. There was also in Saint-Simon an embryonic theory of history—history as a study of the scientific laws governing human development. As Mr. Markham points out in his introduction, there is hardly a single element in Saint-Simon's thought which cannot be traced to another source: but the peculiar synthetic quality of his mind transforms these elements into something new, something which recognizably belongs to the nineteenth—and often to the twentieth—century, but certainly not to the age in which he lived.

It should be noted that Saint-Simon was led to these ideas by the cult of science and of the scientific method which he in fact inaugurated. This cult was stated in a fantastically exaggerated form in his first publication, *Letters from an Inhabitant of Geneva* (1803), in which he advocated the setting up of a Council of Newton composed of 21 scientists and artists, which would supersede the spiritual authority of Papacy and unify all knowledge, including that of social relationships, on the basis of Newton's law of universal gravitation. Apart from establishing the spiritual reign of this top class of scientists, artists and men of genius, the Council would also have to reconcile the interests of the other two classes which, according to Saint-Simon's view, make up society: the "haves" and the "have-nots" between which, in the nature of things, there always and inevitably exists a struggle. The exhortations to the three classes culminate in a famous passage in which Saint-Simon preaches the cult of work ("all men will work" and "anybody who does not obey the orders will be treated by the others as a quadruped") and from which much of his later doctrine springs.

The ideas thus stated in an embryonic form were more fully developed in the *Introduction to the Scientific Studies of the Nineteenth Century* (1808) and the *Essay on the Science of Man* (1813). The former is, in Professor Hayek's view, a remarkable document: it combines for the first time nearly all the characteristics of the modern scientistic organizer and marks "the beginning of both modern positivism and modern socialism, which, thus, both began as definitely reactionary and authoritarian movements". In the latter, Saint-Simon conceives not only the name but the programme of

positivism, later to be carried out by Comte. In both these tracts can already be seen an original outline of a theory of history.

The *Essay on the Science of Man* marks the end of Saint-Simon's own unaided thought. From that time dates the beginning of the Saint-Simonian school in whose works it is almost impossible to distinguish the exact part of the master and that of the pupils, who were sometimes his intellectual superiors. The first of these helpers, who figures on the title-page of the master's next work, *The Reorganization of the European Community* (1814), was Augustin Thierry, later to become the leader of the new school of historians who saw history in terms of the masses and of class struggles, and thus profoundly influenced Marx. It was during this period that Saint-Simon's enthusiasm for science partly gave way, logically enough, to his new enthusiasm for industry. With the help of bankers and industrialists he started a journal, *L'Industrie,* in which he not only expressed his desire to terminate the revolution, but also formulated the idea of the subordination of politics to economics and not vice versa.

But perhaps the most remarkable of Saint-Simon's writings are to be found in another journal with the significant title of *Organizer,* which he started with the most famous and the most influential of his pupils, Auguste Comte. Here is sketched a real plan for the reorganization of society and of the political system on the basis of modern industrial and economic planning. The direction and execution of this planning were to be placed in the hands of "industrialists", by which he meant all those who do productive work at a high level, i.e. engineers, artists, scientists, and the richest and most successful entrepreneurs. These ideas were further developed in two other works written in collaboration with Comte, the *Industrial System* (1821) and the *Catechism of Industrialists* (1823), which marked an appreciable move towards authoritarian socialism. In the latter work, Comte sketched the outline of "Social Physics" which he later called "Sociology". Soon afterwards Comte broke with Saint-Simon when the latter began to turn his doctrine into a religion. The product of this last and mystical phase was Saint-Simon's ultimate work, the *New Christianity,* which appeared a few weeks before his death in 1825 but added nothing to his previous ideas beyond turning the positivist system into a new religion.

It has been said that Saint-Simonism was born after Saint-Simon's death, and that the real force which decisively influenced European thought were the Saint-Simonians and not Saint-Simon himself. Less than a month after the master had died his friends and disciples formed an association which counted among its members Rodrigues, Enfantin, Bazard, Transon, Lechevalier and scores of other Polytechnicians. The course of lectures which they arranged in 1829-30 and which was printed as *Doctrine de Saint-Simon, Exposition* is "by far the most important document produced by Saint-Simon or his pupils and one of the great landmarks in the history of socialism". If it is not the Bible of socialism, it deserves at least, in Professor Hayek's view, to be regarded as its Old Testament.

The influence which the Saint-Simonian movement exercised on European thought was tremendous, and the tracing of this influence is perhaps one of the most interesting parts in Professor Hayek's masterly account. George Sand, Balzac, Victor Hugo in France, Carlyle and John Stuart Mill in